VOL. XVIII, NO. 4 APRIL, 1924

# THE SCIENTIFIC MONTHLY

*EDITED BY J. McKEEN CATTELL*

CONTENTS



THE SCIENCE PRESS
LANCASTER, PA. GARRISON, N. Y.
NEW YORK, N. Y., Grand Central Terminal
Single Number, 50 Cents Yearly Subscription, $5.00

Entered as second-class matter July 18, 1923, under the Act of March 3, 1879

# THE SCIENTIFIC MONTHLY

APRIL, 1924


## LIFE HISTORY OF AN ALPHA PARTICLE FROM RADIUM[1]


By SIR ERNEST RUTHERFORD

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION; CAVENDISH PROFESSOR OF EXPERIMENTAL PHYSICS, UNIVERSITY OF CAMBRIDGE


In this lecture I propose to discuss some of the properties of the high-speed $\alpha$-particle which is ejected spontaneously from radioactive substances. This flying atomic nucleus is not only the most energetic projectile known to us, but it is also an agent of great power in probing the structure of atoms, so that an account of the effects produced by it is of wide scientific interest.

It is now well established that the $\alpha$-particle expelled from radioactive bodies is in all cases a helium atom, or, to be more precise, the nucleus of a helium atom of mass 4 carrying two positive charges of electricity. It is only when the expelled nucleus is stopped by its passage through matter that it captures the two negative electrons required to convert it into the neutral helium atom. It is natural to suppose that the helium nucleus, which is shot out at great speed from the heavy nucleus of a radioactive atom, formed part of its structure. For some reason, which is not as yet understood, occasionally one of the radioactive nuclei breaks up with explosive violence, ejecting the component helium nucleus with high velocity. It is probable that the $\alpha$-particle in escaping from the radioactive nucleus acquires part of its great energy of motion in passing through the repulsive electric field surrounding the latter, but at present we do not know the nature of the forces which hold the complex nucleus together, or whether the $\alpha$-particle is at rest or in orbital motion in the nuclear structure before instability sets in. We know, however, that there is a very wide range of stability exhibited by different radioactive elements. In a sub-

[1] Address before the Royal Institution of Great Britain.

stance like radium A the average life of the radioactive atom before ejection of an α-particle is about 4.3 minutes, for radium itself 2,250 years, while in the case of a very slowly changing element like uranium the average life is of the order of 7,000 million years.

It is known that the α-particles from a given element are all shot out with the same speed, but that this speed varies from element to element. There is apparently a close connection between the velocity of ejection of the α-particle and the average life of the parent element. The shorter the average life of the element, the swifter is the speed of expulsion. This interesting relation between the violence of the explosion and the average life of the element holds in the majority of cases, but it is difficult at present to be at all clear of its underlying meaning. Sir William Bragg long ago showed that the α-particle travels through matter nearly in a straight line, and has a definite range of travel in a substance. This is well illustrated by the tracks of α-particles obtained by Wilson's expansion method. The majority of the tracks are seen to be quite straight, apart from an occasional deflection near the end of the path. At the end of the range the photographic and ionizing effects of the α-particle apparently cease with great suddenness. On account of its great energy of motion, the individual α-particle can be detected by the scintillation it produces in crystalline zinc sulphide, by the effect on a photographic plate, and by special electrical methods, while the beautiful expansion method of Wilson shows the trail of each individual α-particle through the gas.

We are enabled, particularly by the scintillation method, to count the individual particles, and thus we have at our command a method of great delicacy for studying the effects produced by the passage of α-particles through matter. In travelling through a gas the α-particle passes the outer electronic structure of a large number of atoms and liberates electrons, thus giving rise to an intense ionization along the track. The ionization increases to a maximum near the end of the path of the α-particle and then falls rapidly to zero.

A careful study has been made of the law of decrease of velocity of the α-particle in passing through matter by studying the deflection in a magnetic field of a pencil of α-particles before and after its passage through a known thickness of matter. In most of these experiments we employ the α-particles of radium C, which have a range of about 7 cm in air under ordinary conditions. The initial velocity $V_0$ of these particles is known to be 19,200 kilometers per second, and the reduction of velocity can readily be followed down to about 0.4 $V_0$. At this stage the emergent range of the α-particles is less than one centimeter, and measurements are difficult, owing to the fact that a beam of α-particles becomes heterogeneous and contains particles moving with different velocities.

For this reason the velocity of the α-particle can not be followed with certainty below 0.38 $V_0$. We must bear in mind that even at the lowest velocity at which it is possible to detect the α-particle by the scintillation or photographic method, it is still moving at a high speed compared with the positively charged particles generated in an ordinary discharge tube.

It is clear that ultimately the α-particle must be slowed down to such an extent that it captures electrons and becomes a neutral atom, but until recently no evidence of this process of capture of electrons had been obtained. G. H. Henderson[2] has recently added much to our knowledge of this subject by examining the deflection of α-rays in a magnetic field in a very good vacuum. For the success of these experiments it is essential that the apparatus in which the deflection is observed should be exhausted to a very low pressure, corresponding to that required for a good X-ray tube. The reason of this will be seen later. When a narrow pencil of α-rays was deflected in a magnetic field two bands were observed on the photographic plate: one the main band, due to ordinary α-particles carrying two positive charges, and another midway band, which he supposed to consist of particles which had captured one electron, *i.e.*, to singly charged helium atoms. At low velocities he also obtained evidence of the existence of neutral α-particles resulting from the capture of two electrons by the helium nucleus. In these experiments Henderson employed Schumann plates, where the film is so thin that low velocity particles produce as much or more photographic effect than the swifter particles.

I have repeated these experiments by the scintillation method, and confirmed the deduction of Henderson. By observing the

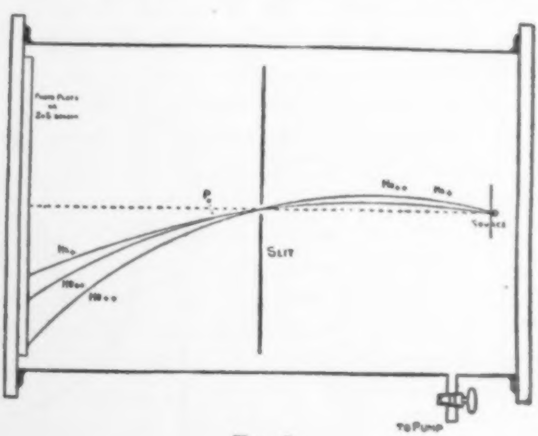



FIG. 1

[2] Proc. Roy. Soc., A, cii, p. 496, 1922.

deflection of the midway band in an electric as well as in a magnetic field, I find there is no doubt the particles composing the midway band consist of particles of mass 4 and charge 1—*i.e.*, to singly charged helium atoms which have the same speed as the doubly charged particles comprising the main band.

Some recent experiments have been made by me to throw light on the conditions under which the flying $\alpha$-particles may gain or lose an electron. The general arrangement of the experiment is shown in Fig. 1. A fine platinum wire coated with radium B + C, by exposure to the emanation (radon), serves as a nearly homogeneous source of $\alpha$-rays, since the $\alpha$-particles are emitted only from the atoms of radium C, which are too few in number to form a film on the platinum of even one molecule thick. The $\alpha$-rays from this source pass through a narrow slip about 0.3 mm wide and fall on a screen of zinc sulphide. The distribution of $\alpha$-particles on the screen is determined by the scintillation method in a dark room, using a microscope outside the box. The vessel containing the source and screen is completely exhausted by means of a Gaede and mercury diffusion pump, and if necessary the residual pressure can be measured by a Macleod gauge. The box is placed between the plane pole pieces of a large electromagnet so that the pencil of $\alpha$-rays is bent in the direction shown in the figure. Usually the distance between the source and screen was 16 cm, with the slit midway. The whole path of the rays was exposed to a nearly uniform magnetic field, and the deflection of the pencil of rays was proportional to the strength of the magnetic field. Under normal experimental conditions the pencil of $\alpha$-rays from the bare radium C wire was bent a distance on the screen of about 15 mm from the zero position without field. The field of view of the microscope was sufficient to take in the depth of the whole pencil of $\alpha$-rays without the field.

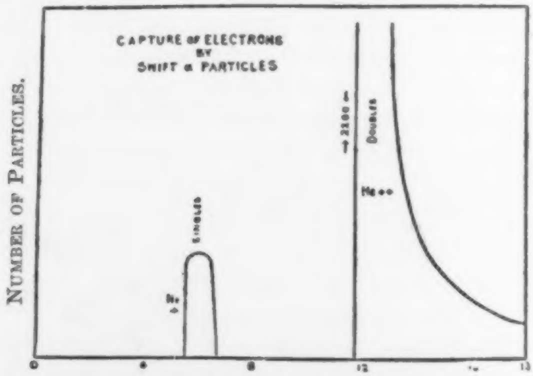



FIG. 2.—DEFLECTION IN MM BY MAGNETIC FIELD

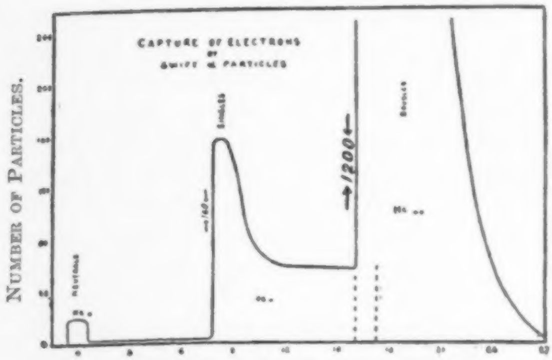



FIG. 3.—DEFLECTION IN MM BY MAGNETIC FIELD

Special precautions were taken to prevent contamination of the screen by the escape of active matter from the wire in a low vacuum. It must be borne in mind that the type of wire source employed always introduces some heterogeneity in the beam of α-rays even from the uncovered source. This is due to the escape from the back of the wire of α-particles which are reduced in velocity in passing through the material. This effect is clearly manifest when the pencil of α-rays is deflected by a magnetic field; for in addition to the main band of α-rays there is always a distribution of particles extending beyond the main beam. The intensity of this heterogeneous beam at any point is generally less than 1 per cent. of the main beam, and does not seriously interfere with the accuracy of the deductions discussed in this lecture.

In Figs. 2 and 3 are given illustrations of the distribution of singly and doubly charged α-particles along the zinc sulphide screen. Fig. 2 shows the result when a thickness of mica corresponding in stopping power to 3.5 cm of air is placed over the source. The main band, due to $He_{++}$ particles, is sharply defined on the high velocity side, but there is evidence of some heterogeneity produced in the beam by its passage through the mica. As we should expect, the midway band ($He_{+}$ particles) lies exactly between the zero position and the main band, and contains only about 1/55 of the particles in the main beam. Fig. 3 shows the distribution when the thickness of mica is increased to correspond to a stopping power of about 6 cm of air. Both the main and midway bands are no longer sharply defined as in the first case, but each consists of particles with a considerable range of velocities. The relative number of $He_{+}$ and $He_{++}$ particles is about ⅛ for the swifter particles, but this ratio increases with decreasing velocity. The midway band extends and joins the main band where it can no longer be followed. The brightness of the scintillations due to $He_{+}$ particles falls off ob-

viously and continuously from A to B. At this stage, too, some neutral particles make their appearance. This is shown by the $He_0$ band, which is not deflected by a magnetic field, but its intensity is small compared with that of the midway band. There is also a sparse distribution of faint particles between the neutral and midway band, probably due in part to scattering of the α-particles by the edges of the slit and possibly in part due to recoil atoms of oxygen and other elements constituting the mica. The distribution of the charged and uncharged helium particles for a still lower velocity will be seen in curves A, B, Fig. 4, which will be referred to later. It is seen that the relative number of $He_+$ to $He_{++}$ particles has increased; similarly, the relative number of neutral particles is much greater.

We may now consider the interpretation to be placed on these observations. It is clear that the particles emerging from the mica consist of doubly charged, singly charged, and neutral particles, but the relative number of these three types varies markedly with the stopping power of the mica plate. We may suppose that the α-particle in passing through the outer electron structure of the atoms in its path occasionally removes and captures an electron. This electron falls into a stable orbit round the doubly charged helium nucleus and moves with it.

This singly charged atom will, however, have only a limited life, for in passing through other atoms the electron is knocked off and the singly charged α-particle reverts back to the doubly charged type. This process of removal is analogous to the ordinary process of ionization where an electron is ejected from an atom by a collision with an α-particle; for as a singly charged particle can remove electrons from another atom, so there is a chance that the $He_+$ particle should lose its attendant electron. We may thus consider that two opposing processes are at work, one resulting in the capture of an electron and the other leading to its removal. From the data

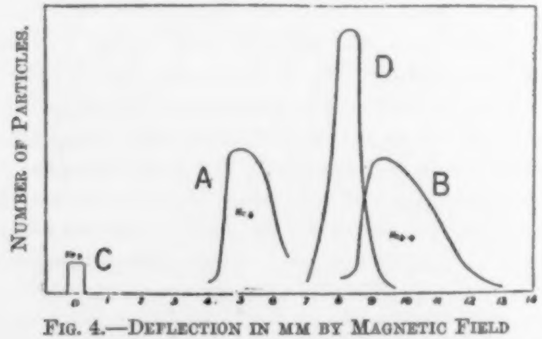


FIG. 4.—DEFLECTION IN MM BY MAGNETIC FIELD

given later it will be seen that this process of capture and loss may repeat itself more than a thousand times in the flight of an $\alpha$-particle, so that the average path travelled by an $\alpha$-particle before capture of an electron or before loss of the captured electron is small compared with the total distance of travel of the $\alpha$-particle before it comes to rest. It is clear from this, for a given velocity of $\alpha$-particle, that there must be a momentary equilibrium between the number of $He_+$ and $He_{++}$ particles such that, on the average, the number of captures in a given small distance is equal to the number of losses.

It is very convenient to suppose that for a given velocity each $He_{++}$ particle has a mean free path $\lambda_1$ cm in the material before it captures an electron, and the $He_+$ particle a mean free path $\lambda_2$ cm before it loses its attendant electron. No doubt some of the individual particles travel distances much shorter or longer than this mean distance before either capture or loss, but in considering a large number of particles we may suppose there is an average distance traversed before capture or loss, to be called the mean free path.

When $N_1$ $He_{++}$ particles traverse a small distance $dx$ of a material the number which capture electrons is $N_1 dx/\lambda_1$. If $N_2$ $He_+$ particles are present the number which lose an electron is $N_2 dx/\lambda_2$. But we have seen that when an equilibrium is set up, the number of captures in a given distance must equal the number of losses. Equating these two expressions, it is seen that $N_2/N_1 = \lambda_2/\lambda_1$, or, in other words, the relative number of $He_+$ to $He_{++}$ particles is proportional to the ratio of the mean free path for loss to that for capture. Since by the scintillation method the ratio $N_2/N_1$ can be measured for any velocity, by using different thicknesses of absorber we can thus determine the ratio of the mean free paths for capture and loss for any velocity.

The actual value of the mean free path $\lambda_2$ of the $He_+$ particle before it loses its electron can be directly determined by experiment. Suppose the microscope is focussed on the midway band of Fig. 2 and the number of scintillations per minute observed in a good vacuum. If the pumps are shut off and a small quantity of air or other gas is introduced into the apparatus, the number of scintillations is found to diminish with increasing pressure of the air until the band has completely disappeared. This takes place at quite a low pressure of air: for example, for a pressure of about ¼ mm in the box.

The explanation of this result is obvious. The $He_+$ particles which escape from the mica occasionally collide with an atom of the gas in its path, and the electron which it captured in passing

through the mica is removed. In such a case the $He_+$ becomes again an $He_{++}$ particle, and the latter is twice as easily deflected in a magnetic field as the former. Suppose the collision occurs for the first time at the point P (Fig. 1). The particle after losing its electron travels along a new path shown in the figure, and the particle no longer strikes the part of the screen viewed by the microscope. It is found that the number of scintillations seen in the microscope falls off according to an exponential law as the pressure of the gas is raised. Such a result is to be expected, and from this data the average distance which the $He_+$ particle traverses before it loses its electron can be simply deduced. Certain small corrections are necessary to take into account the finite width of the band of scintillations as seen in the microscope, but we need not enter into details at this stage. It is convenient to express the mean free path $\lambda_2$ in air of the $He_+$ particles, not as the average length of path traversed in the rarefied gas before loss, but as the distance traversed in the same gas at standard pressure and temperature. For example, in a certain experiment the mean free path in air of the particle was found to be 12 cm at a pressure of 0.040 mm; this corresponds to a mean free path of 0.0063 mm at standard pressure and temperature.

In this way the mean free path in air before loss of an electron has been measured for different velocities, and it has been found over a considerable range that the mean free path varies directly as the velocity of the α-particle, so that the mean free path becomes shorter as the velocity of the α-particle diminishes. Since we may regard the loss of an electron from the singly charged particle as the result of a process of ionization, such a relation is to be expected, and indeed, if we take into account the strong binding of a single electron by the $He_{++}$ nucleus, the mean free path for loss is of the same order as that calculated from considerations of the number of ions per cm produced by the α-particle in air and other gases. Comparisons have been made of the mean free path in air with that in hydrogen and helium. Its value is 4 to 5 times longer in hydrogen and more than 5 times longer in helium.

Now that the mean free path $\lambda_2$ is known, the value of $\lambda_1$ for capture can be deduced if the ratio $N_2/N_1$ is also known. A difficulty, however, arises at this point. In order to measure the ratio $N_2/N_1$ it is necessary that the active source should be covered with mica or other solid material. Gas can not be used conveniently. It was found, however, that the ratio $N_2/N_1$ was the same within the limits of error whether the α-particles were reduced in velocity by passage through celluloid, mica, aluminium or silver. For this purpose the mica was kept the same and a very thin sheet of the substance under examination spread over it. The thickness of the sheet was suffi-

cient to set up a new equilibrium between the singly and doubly charged particles, but not sufficient to alter materially the velocity of the ionizing rays.

Since the value of the ratio $N_2/N_1$ suffers no appreciable change for absorbers of such different atomic weights, we may safely conclude that the ratio for a hypothetical sheet of solid air would be the same as for mica.

We have now all the data required to determine the values of $\lambda_1$ and $\lambda_2$ corresponding to $\alpha$-particles of different velocities. The results are given in the following table for three different velocities. The mean free paths are expressed in terms of millimeters of air at standard pressure and temperature. $V_0$, the maximum velocity of the $\alpha$-particles from radium C, is $1.9 \times 10^9$ cm per second.

| Velocity V in terms of $V_0$ | $\lambda_2/\lambda_1 = N_2/N_1$ for Mica | Mean Free Path $\lambda_2$ for Loss in Air | Mean Free Path $\lambda_1$ for Capture in Air |
|---|---|---|---|
| 0.94 | 1/200 | 0.011 mm | 2.2 mm |
| 0.76 | 1/67 | 0.0078 mm | 0.52 mm |
| 0.47 | 1/7.5 | 0.0050 mm | 0.037 mm |

It has been seen that the mean free path for loss varies directly as the velocity, and thus only alters in a ratio of about 1 to 2 over the range of velocities given in the table. On the other hand, the ratio $\lambda_2/\lambda_1$ increases very rapidly with diminution of velocity varying approximately as $V^{-5}$. From this it follows that $\lambda_1$ varies as $V^6$, thus decreasing by a factor of 60 or more when the velocity is halved.

From these data and relations it can easily be calculated that the mean free path for capture should be equal to that for loss for a velocity about 0.3 $V_0$, and for this speed the numbers of $He_+$ and $He_{++}$ particles should be equal.

The actual value of the velocity for equality of the two types in a special experiment was found to be 0.29 $V_0$, in good agreement with the calculated value. It is a difficult matter to determine the values of $\lambda_1$ and $\lambda_2$ for velocities less than 0.3 $V_0$, for not only are the scintillations weak in intensity and difficult to count with accuracy, but also the issuing rays are very heterogeneous and no longer show well-defined edges on the high velocity side. It was, however, noted that the ratio $N_2/N_1$ rapidly increased below the velocity 0.3 $V_0$.

We have so far dealt with the equilibrium between $He_+$ and $He_{++}$ particles. It is clear, however, that similar considerations apply to the equilibrium between singly charged and neutral helium

particles at low velocities of the $\alpha$-particle. It was noted that the neutral particles appear prominently after the rays have passed through mica of 6 cm stopping power, but no doubt they could be detected for still lower stopping power. These neutral particles, of course, produce scintillations, but of an intensity corresponding to an $\alpha$-particle of low velocity. These neutral particles probably lose and regain an electron many times before they are stopped in the zinc sulphide or other absorbing material. This effect was shown by introducing gas at low pressure into the apparatus, when the scintillations due to the neutral particles diminished in number and ultimately vanished. The explanation of this is similar to that given for the disappearance of the $He_+$ band, for the neutral particles occasionally lose an electron in passing through the gas and are then deflected away from the zero position by the magnetic field.

It was estimated that the mean free path in air for conversion of neutral helium particles to singly charged particles was about 1/600 mm. No doubt this is an average for particles of very different velocities which may be present in the neutral band.

For the higher velocities we have to deal mainly with the interchange $He_{++} \rightleftarrows He_+$. For velocities less than 0.5 $V_0$ the interchange $He_+ \rightleftarrows He_0$ also comes in and becomes all-important for velocities less than 0.3 $V_0$. No doubt, as Henderson has shown, at still lower velocities most of the $He_{++}$ particles disappear and the $He_0$ and $He_+$ particles predominate.

At these low velocities counting scintillations becomes very difficult and uncertain, and the photographic method, as used by Henderson, is preferable. It will be a matter of very great interest to examine whether the relative numbers of the three types of particles alter when the $\alpha$-particles are slowed down by passage through different materials. This side of the work is being attacked by Mr. Henderson in the University of Saskatchewan.

There is one very interesting point that may be considered here. It has been shown that these singly and doubly charged $\alpha$-particles are always present after the $\alpha$-rays have passed through mica or other absorber; but are there any singly charged particles present when $\alpha$-particles escape from a wire coated with an infinitely thin deposit of active matter? This was first tested for a platinum wire coated with a deposit of radium B + C by exposure to the radium emanation, when it was found that singly charged helium atoms were present in about the equilibrium ratio for this velocity. This was a rather surprising observation, but it was thought it might result from the fact that by the recoil from radium A the radium B particles penetrate some distance into the material of the wire. Under these conditions many of the $\alpha$-particles expelled from

radium C have to pass through a small but appreciable thickness of matter before escape from the wire, and might thus capture electrons. This explanation seemed unlikely because the average distance penetrated by the recoil atom is only a minute fraction of the mean free path for capture at such high velocities of the α-particle. The experiment was tried with a nickel wire on which radium C had been deposited on the surface by the well-known method of dipping the wire in a hot solution of radium C. In this case the difficulty due to recoil is absent, but the number of singly charged particles was the same as before.

It is very significant that the relative number of singly and doubly charged particles is about the equilibrium ratio to be expected when the wire, after being activated, is coated with an appreciable thickness of copper or other material. We can scarcely suppose that singly as well as doubly charged particles are actually liberated from the radioactive nucleus itself, for even if it be supposed that an α-particle with an attendant electron is expelled, the electron must be removed in escaping through the very powerful electric field close to the nucleus. It is much more probable that the doubly charged α-particle in passing through the dense distribution of electrons surrounding the radioactive nucleus occasionally captures an electron, and that the process of capture and loss goes on to some extent in escaping from the radioactive atom. This seems at first sight rather unlikely when we consider the relatively large number of atoms an α-particle ordinarily passes through before equilibrium between capture and loss is established, but it is well known that the chance of effective electronic collisions appears in general to be greater for a charged particle expelled from the central nucleus than for a similar particle passing from outside through the electronic distribution of an atom. It may be that those electrons the orbital motion of which round the nucleus is comparable with the speed of the α-particle are particularly effective in causing capture or loss.

So far we have dealt mainly with the distribution in a magnetic field of the particles in a vacuum after their escape from a mica surface. Some very interesting points arise when the distribution is examined in the presence of sufficient gas to cause a rapid interchange of capture and loss along the path of the α-particle in the gas. This is best illustrated by a diagram, Fig. 4, in which the results are given for α-particles escaping through mica with a maximum emergent range of about 4 or 5 millimeters in air. Curves A and B give approximately to scale the distribution of $He_+$ and $He_{++}$ particles in a vacuum, while C gives the relative number of neutral particles under the experimental conditions. Suppose

now sufficient air is introduced into the vessel to cause many captures along the gas, but yet not enough to reduce seriously the velocity of the α-particles. The first salient fact to notice is that the distributions A, B, C vanish and there remains a distribution of particles (curve D) about midway between A and B. This band is narrower than either A or C, and its height at the maximum much greater than either. It is evident that the particles have been compressed into a band of much narrower width than the normal distribution in curve B.

This is exactly what we should expect to happen. The swifter particles present suffer less capture than the slow; consequently the average charge of the swifter α-particles along the gas is less than $2e$, and their deflection is less than the swiftest particles shown in curve B. On the other hand, the slower α-particles have an average charge nearer $1e$ than $2e$, and are relatively still less deflected than the swifter particles. It is thus clear that the resulting distribution of particles with air inside the vessel will be concentrated over a much narrower width than the main band of $He_{++}$ particles. From calculation based on the laws of capture and loss, the width of the band under the experimental conditions can be deduced, and is found to be in good accord with experiment. It will be seen to be significant that similar results have been observed for hydrogen under corresponding conditions.

## General Discussion of Results

Attention may now be devoted to a consideration of the results so far obtained and the possibility of their explanation on present views. In the first place, it is important to emphasize the large number of capture and losses that occur during the flight of an α-particle from radium C. While the mean free path of the α-particle from radium C of 7 cm range is about 3 mm in air, its value rapidly decreases with lowering of the velocity of the α-particle, and is probably about 0.0015 mm for a velocity of $0.3\ V_0$. It is not difficult to calculate that not far short of a thousand interchanges of charge occur during the path in air of a single particle between velocities $V_0$ and $0.3\ V_0$. While the data so far obtained do not allow us to calculate the number of interchanges of charge that occur between velocities $0.3\ V_0$ and 0, it seems probable that the number is considerably greater than a thousand. We have already pointed out that for low velocities the interchange $He_+ \rightleftarrows He_0$ predominates. When we consider the rapidity of interchange of charges of the α-particle at average velocities, it seems clear that we can not expect to observe any appreciable difference in power of penetration between a beam of rays of the same velocity, whether

consisting initially of singly or doubly charged particles. It is clear that a singly charged particle after penetrating a short distance is converted into a doubly charged particle, and vice versa, and that the effects due to the two beams should be indistinguishable. Henderson tried such absorption experiments, using the photographic method, but with indefinite results.

When an $\alpha$-particle captures an electron, the latter presumably falls into the same orbit round the helium nucleus as that which characterizes an ionized helium atom, *i.e.*, an atom which has lost one electron. When the $\alpha$-particle with its attendant electron passes swiftly through the atoms of the gas in its path, it will not only ionize the gas but will also occasionally be itself ionized, *i.e.*, will lose its attendant electron. When we take into account the strong binding of the first electron to the helium nucleus—ionization potential about 54 volts—the mean free path for loss of the captured electrons in air is of the right order of magnitude to be expected from considerations based on the ionization by the $\alpha$-particle per unit path in air. While we can thus offer a quantitative explanation of the mean free path for loss observed experimentally, the inverse problem of the capture of an electron by the flying $\alpha$-particle presents very great difficulties.

In the actual case, the $\alpha$-particle is shot at high speed through gas molecules which for all practical purposes may be supposed to be at rest. For convenience of discussion, however, it is preferable to make an equivalent assumption, namely, that the $\alpha$-particle is at rest and the gas molecules stream by it with a velocity equal and opposite to that of the $\alpha$-particle. Now the maximum velocity of an $\alpha$-particle from radium C is equivalent to that gained by an electron in falling freely between a difference of potential of about 1000 volts; so that the electrons comprising the molecules of air or other gas have a velocity of translation numerically equal to this. For brevity, it is very convenient to speak of this velocity or energy as that due to a "1000-volt" electron.

When the electrons in an atom pass close to the $\alpha$-particle, one of them may be removed from the parent atom by the collision, energy being required for this process. The ionization potential for oxygen or nitrogen is about 17 volts, which is a very small quantity compared with the energy of translation of a 1000-volt electron.

If we consider the forces involved between an $\alpha$-particle and moving electron as of the ordinary electrostatic type, the electron will describe a hyperbolic orbit round the nucleus, the angle of deflection of the path of the electron resulting from the collision depending on the nearness of the approach of the electron to the nucleus. On ordinary dynamics, the electron will never be captured in such a collision if there is no loss of energy by radiation. If

capture for some reason results from the collision, it means that an amount of energy corresponding to at least a 1000-volt electron has in some way been got rid of. This loss of energy may be supposed to be due to some interaction between the α-particle and colliding nucleus with its attendant electrons, or to the loss of energy by radiation during the collision. The first supposition seems at first sight plausible, for we know that the innermost electrons of oxygen or nitrogen are strongly bound and require energy of the order of 500 volts to remove them from the atom. But there is one very strong and, it seems to me, insuperable objection to this view.

I have found that the deflection in a magnetic field of a pencil of α-particles passing through a suitable pressure of hydrogen is similar to that shown in curve D, Fig. 4, for air. This shows that the α-particle passing through hydrogen captures electrons of energy about 120 volts to about the same degree as in air. Now we know that the electrons in the hydrogen atom or molecule are lightly bound, and an energy of not more than a 30-volt electron, suitably applied, would entirely separate the component nuclei and electrons in the hydrogen molecule. In the case of hydrogen, therefore, we can not hope to account for the requisite loss of energy, which for the experiment considered is about 100 volts. If these experiments with hydrogen are correct, and are valid for all velocities of the α-particle, we are driven to conclude either that some unknown factors are involved in the capture, or that the loss of energy of the electron must be ascribed to radiation. In such a case capture of an electron may be regarded as the converse of the photo-electric effect, where radiation falls on matter and swift electrons are ejected from the matter. In the case under consideration swift electrons are shot towards a charged nucleus and an occasional electron is captured with the emission of energy in the form of radiation. On such an hypothesis the radiation of energy from an α-particle passing through a gas due to the frequency of capture is very great, amounting to about 3 per cent. of the total energy of the α-particle. This seems to be an unexpectedly large amount, but can not be ruled out as impossible in the present state of our knowledge.

In the discussion of this very thorny question, I have confined myself mainly to the case of capture by the swift α-particle, where the difficulties of explanation are much greater than for capture at slower velocities. Our information is at present too incomplete to give a decisive answer, but there seems to be no doubt that the unexpected frequency of capture of electrons by swift α-particles raises many new and interesting questions of the nature of the processes that can occur in collisions between electrons and matter.

I need scarcely say that the phenomena of capture and loss are not confined to the α-particle, but are shown by all charged atoms in

swift motion through a gas, and were long ago observed in the case of positive rays. On account, however, of the high velocity of the α-particles and the ease of their individual detection, the process of capture and loss can be studied quantitatively under simpler and more definite conditions than in the case of the electric discharge through a gas at low pressure.

On this occasion I have devoted my attention to the most recent additions to our knowledge of the life history of the α-particle. This knowledge has been obtained from the study of the rapid interchange of charges when an α-particle passes through matter. I have only incidentally referred to the numerous collisions with electrons along the track of the α-particle which result in dense ionization. I have omitted any consideration of those rare but interesting encounters in which an α-particle is deflected through a large angle by a close collision with a nucleus. I have omitted, too, the still rarer encounters that may result in a disintegration of an atomic nucleus like that of nitrogen or of aluminium. We have seen that an α-particle has an interesting history. Usually it is retained as an integral and orderly part of a radioactive nucleus for an interval of more than a thousand million years. Then follows a cataclysm in the radioactive nucleus; the α-particle gains its freedom and lives an independent life of about one hundred millionth of a second, during which all the incidents referred to in this lecture occur.

If we are dealing with a dense and compact uranium or thorium mineral, the α-particle after acquiring two electrons and becoming a neutral helium atom may be imprisoned in the mineral as long as the mineral exists. The occluded helium can be released from the mineral by the action of high temperature, and after removal of all other gases can be made to show its presence by the characteristic brilliant luminosity under the stimulus of the electric discharge. In the circumstances of such an experiment only small quantities of helium are liberated. Large quantities of helium, sufficient to fill a large airship, have, however, been isolated from the natural gases which escape so freely from the earth in various parts of Canada and the United States. It is a striking fact that every single atom of this material has in all probability had the life history here described.

## Addendum[3]

It may be of interest to give here a brief review of some additional facts in connection with the α-particle, brought to light in recent years. It has long been known that α-particles, although projected from the source at the same speed, travel unequal dis-

[3] This did not form part of the discourse, but it may usefully supplement one or two of the points surveyed in the lecture.

tances through a gas. For example, the maximum distance travelled by the α-particles from radium C in air is 7.04 cm at 760 mm and 15° C., the minimum distance is about 6.4 cm, and the mean distance about 6.8 cm. Some "straggling" of the α-particles is to be anticipated on general grounds, since the α-particle loses its energy mainly in liberating electrons from the atoms of matter in its path. On the laws of probability one α-particle may meet more atoms and liberate more electrons than another, and thus lose energy at a faster rate. The amount of straggling observed is, however, much greater than can be accounted for in this way, and the occasional large deflections of the α-particles due to nuclear collisions are so rare, except near the end of the range, that they do not seriously influence the final distribution.

Henderson has suggested that the property of an α-particle of capturing and losing electrons will introduce a new factor in causing straggling. No doubt this is the case, but the rates of capture and loss observed appear to be too rapid to account entirely for the discrepancy between theory and experiment. Another interesting suggestion has been made by Kapitza to account for the magnitude of this straggling. From the experiments of Chadwick and Bieler on the collision between α-particles and hydrogen nuclei, it has been deduced that the α-particle or helium nucleus has an asymmetrical field of force around it. This asymmetry of the electric field must become small at the distance of the orbits of the electrons in the neutral helium atom, but may be sufficient to fix the plane of the orbit of an electron relative to the axis of the helium nucleus.

Suppose that the α-particles liberated from a radioactive source have their axis orientated at random, and that the direction of the axis of each individual particle remains unchanged during its motion. In some cases, for example, the captured electron will describe an orbit of which the plane is nearly in the direction of motion of the α-particle; in other cases nearly perpendicular to it. It is to be expected, however, that the chance of losing the captured electron by collision will be greater in one case than the other; or, in other words, the mean free path of the singly charged α-particle before loss of its electron will be different in the two cases.

On this view it is to be anticipated that one group of α-particles will lose energy faster than the other, and the ranges will be different. In order to test whether α-particles show the individual differences to be expected on this theory, Kapitza has photographed in the Cavendish Laboratory the tracks of a number of α-particles by the Wilson expansion method, using a strong magnetic field of about 70,000 Gauss, produced by a momentary current of great intensity. The magnetic field was sufficiently strong to cause a marked bending of the track of the α-particle. It was found that the curvature of the tracks at equal distances from the ends showed marked varia-

tions. Before any definite decision can be reached a large number of tracks obtained in this way must be carefully measured up and allowance made for the sudden bends which occur due to a nuclear collision with the atoms of nitrogen or oxygen. The frequency of these bends near the end of the range complicates the interpretation of the apparent curvature which is measured. The experiments, which are still in progress, are difficult and require great technical skill, and it will be a matter of much interest if any definite asymmetry in the orbits of the singly charged $\alpha$-particles can be established by this or other methods. If such an asymmetry exists, it must influence to a small extent the arrangement of the two electrons round the helium nucleus and possibly their spectrum.

During the past two years Blackett, in the Cavendish Laboratory, has made a careful examination of the frequency of occurrence of sharp bends or forks in the tracks of $\alpha$-particles near the end of their range in air and other gases. For this purpose a simple form of Wilson expansion chamber, of the type designed by Shimizu, has been used, and each track has been photographed in two directions at right angles to each other to fix the angle of the forks in space. A large number of photographs have been taken, and the frequency of the forks has been examined in different gases, particularly in the last centimeter of the range of the $\alpha$-particle. Assuming that these forks arise from nuclear collisions, it is possible to deduce from the experimental data the variation of velocity of the $\alpha$-particle near the end of its range. It is known from the work of Geiger and Marsden that the maximum velocity $v$ of the $\alpha$-particles of emergent range $R$ is given by $v^3 \propto R$, when $R$ is not less than one centimeter. Blackett finds that this relation between velocity and range no longer holds near the end of the track, but is replaced by a relation of the form $v^{1 \cdot 5} \propto R$.

In the course of these experiments a number of well-defined forks have been photographed in hydrogen, helium, air and argon by Blackett, and also by Auger and Perrin in Paris. By measuring the angles between the original direction of the $\alpha$-particle and the direction of the colliding particles after collision, the accuracy of the laws of impact can be directly tested. The results are found, within experimental error, to be in agreement with the view that the impacts are perfectly elastic, and that the conservation of energy and of momentum hold in these nuclear collisions. Conversely, by assuming that the impacts are perfectly elastic, it is possible to deduce the mass of the recoil atom in terms of the $\alpha$-particle of mass 4.00. For example, a fork in helium gave the mass of the recoil atom 4.03, and a fork in hydrogen gave the mass of the recoil atom 1.024. In a collision between the $\alpha$-particle and a helium nucleus the angle between the forks should be exactly a right angle; the value measured was 89° 45′.

# ONE EMBRYO FROM TWO EGGS[1]

By Professor T. H. MORGAN

COLUMBIA UNIVERSITY

Eggs are occasionally found that are twice as large as the normal eggs of the species. They are supposed to arise from the union of two eggs. They develop into embryos that are twice as big as normal embryos. The origin of these "giant eggs" is of peculiar interest to embryologists, and their occurrence has led to several attempts to unite two eggs by artificial means.

The inclusion of two yolks in a single shell that is not infrequently observed in hens' "eggs" is quite a different affair. These "double eggs" of the hen are only two eggs (yolks) that have been set free from the ovary at the same time, and have become enclosed in a common albumen and shell. They give rise to two embryos, but these are not united, and both die, as a rule, before hatching, although occasionally one of them—the one nearer the large end of the egg—may survive because it is so placed that this chick may make use of the air in the air chamber at the large end of the egg during the final stages of development.

Fusion of two blastulas of sea urchins has also been observed and even experimentally brought about. The results are less instructive than when the union has taken place before development begins, but the facts are interesting in so far as they furnish evidence as to what extent readjustments can take place after the development has already been carried forward to the blastula stages. In point of time, moreover, these cases were the ones first recorded.[2] An account of these fused embryos will be given elsewhere.

The artificial union of parts of amphibian embryos will also be considered at another time in connection with experiments relating to grafting, since in most of these cases the development has progressed so far that the problem of readjustment involves little more than the actual union of the cut surfaces that are brought together.

[1] Chapters from "Experimental Embryology," IV.

[2] The earliest account of the union of two embryos into monstrous double forms is that of Lacaze Duthier, in 1875. He observed such union in the embryos of the mollusc, *Philine aptera*. Metchnikoff, in 1886, recorded the fusion of two or three blastulas of the hydrozoon *Mitrocoma annae*. Korschelt ('95) states that the eggs in the body cavity of an annelid Ophryotrocha are sometimes fused. The union of the blastula stages of the sea urchin observed by Morgan ('95), Driesch ('00), Bierens de Haan ('13) and Goldfarb ('14) will be considered in another connection.

In Triton, however, Spemann ('16, '18) has succeeded in grafting pieces of blastula stages together, and has succeeded in incorporating a piece of one embryo into another, even when the two belong to different species. These results will also be described in connection with grafting experiments on embryos.

In general it may be said that the results obtained from fused eggs or embryos have not solved any of the larger problems of development, but the results have been useful in studying special problems and have broadened our ideas concerning some of the possibilities of regulation between two systems each alone adjusted to produce only a single individual.

## Double Eggs of Sea-Urchins

The development of giant eggs of sea-urchins has been studied by Boveri ('01, '14), Herbst ('14) and Bierens de Haan ('13). Such eggs (Fig. 2, b) furnish an opportunity to study experimentally an important problem, namely, the relative influence of chromatin and protoplasm in the development of *hybrid* larvae. The giant eggs of the sea-urchin have, as a rule, a single nucleus whose surface is twice that of the surface of the nucleus of the normal egg. Twice the normal number of chromosomes are present. The origin of these eggs is unknown. It has been suggested that they may arise from a failure of the protoplasm of a young germ-cell to divide at a time when its chromosomes divide, or that they arise from the fusion of two germ-cells with subsequent fusion of their nuclei. A double cell, formed in either of these ways, would, it is assumed, grow to double the size of the normal egg. It has also been suggested that failure of one or both of the polar bodies to be extruded would produce an egg with a nucleus of double size—a nucleus with the diploid number of chromosomes, but it does not follow that such an egg would then grow to double size, since the polar bodies are formed only when the growth of the egg has come to a standstill.

Bierens de Haan ('13) records that in certain individuals and in certain years, and at certain time in the year, giant sea-urchin eggs are not so rare as at other times. One female (Sphaerechinus) had hundreds of such eggs, while other individuals had none. These giant eggs may be fertilized, and if the sperm is much diluted polyspermy may be avoided. The cleavage is normal, giving rise, at the 8-cell stage, to the characteristic four micromeres, etc. The embryo develops at the normal rate. Large blastulae and plutei result. The number of cells is the same as in the normal embryo, but the cells are twice as large. A giant blastula of Sphaerechinus, for example, has about 32 mesenchyme cells, and the same number are found in the normal blastula. The normal egg of

Sphaerechinus, according to Baltzer, contains 20 chromosomes; after fertilization 40. The fertilized giant egg contains, according to Bierens de Haan, 60–63 chromosomes. Forty of these have probably come from the egg (diploid) and 20 from the sperm.

Herbst ('14) has studied hybrids that have been produced by fertilizing the giant eggs of Sphaerechinus (Fig. 2, b) by the sperm of Stronglyocntrotus. A normal pluteus of Strongylocentrotus is shown in Fig. 1, a, and of Sphaerechinus in Fig. 1, b. Two hybrids from normal eggs are shown in Figs. 2, $a^1$, $a^2$, and two hybrids from giant eggs in Figs. 2, $b^1$, $b^2$. It is obvious at a glance that the latter are more like the Sphaerechinus type (Fig. 1, b) than like the hybrid from normal eggs (Fig. 2, $a^1$).

Herbst has analyzed in detail the characters of the larval skeleton of these two kinds of hybrids. The skeletons are quite variable, but in nearly every respect the skeleton of the hybrid giant-pluteus is more like that of the Sphaerechinus pluteus than like that of the hybrid from normal sized eggs.

Herbst records more variability in the size of the nuclei in the giant eggs than was observed by Bierens de Haan. His measurements show that there are two, possibly three, categories of nuclei in regard to size. There are giant eggs whose nucleus has twice the volume of those of the normal nuclei, and eggs that have four times the volume of the normal. Possibly the latter, he suggests, are

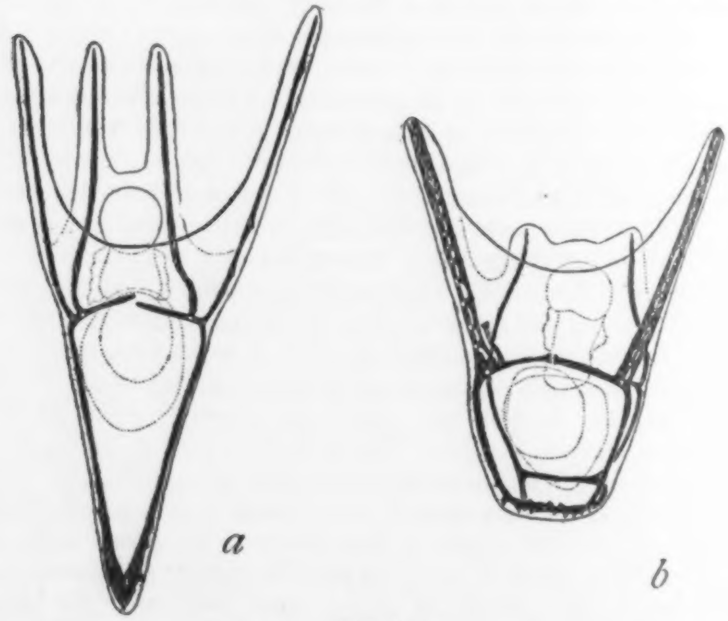



FIG. 1

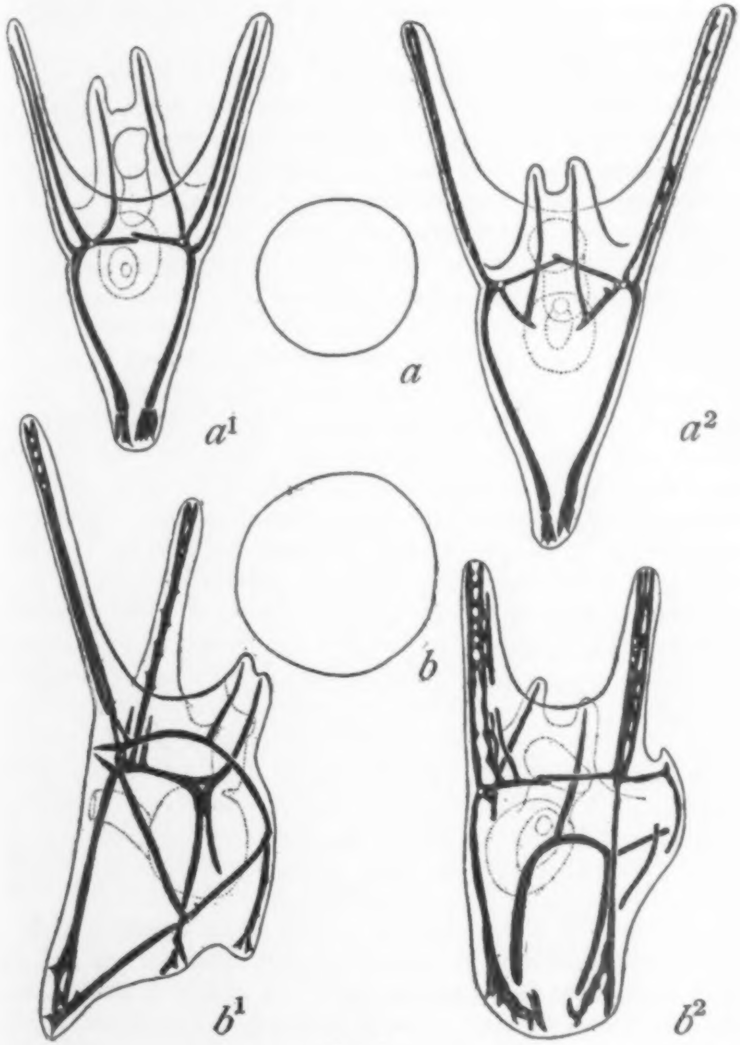



FIG. 2

tetraploid (fourfold) and represent four potential eggs fused together. On the other hand, it is possible that some of the differences are only fluctuations in size of a diploid nucleus. It will require chromosome counts to decide this question, and at present we have only those of Bierens de Haan, that, as far as they go, indicate a diploid condition. Herbst is inclined to interpret the more maternal characteristics of the hybrid giants as due to the larger amount of maternal chromatin in the nucleus. This means that the result depends on the greater influence of the larger num-

ber of the maternal chromosomes, and is in accord with genetic results in general.

Boveri ('13) obtained five giant plutei from giant eggs of Sphaerechinus fertilized by Strongylocentrotus sperm. These, he states, resemble the maternal type of pluteus more than does the hybrid from the normal sized egg. Boveri discusses the problem as to whether the maternal character of these giants is due to the greater quantity of the protoplasm of the egg or to the double-sized nucleus. By means of the following experiment he showed that the amount of the protoplasm does not in itself affect the character of the hybrid. Some normal eggs of Sphaerechinus were broken into fragments. The nucleated fragments were then fertilized by sperm of Strongylocentrotus. Other eggs, not shaken, were cross-fertilized, and then placed in Ca-free sea water. When the two-cell stage was reached, the blastomeres were separated. Both lots were allowed to develop into plutei. An equal number (20) of the same sized plutei of the two lots were compared, *i.e.*, those from the ½ blastomeres were compared with embryos of the same size from the fragments. Both were alike; *i.e.*, neither showed a greater tendency to be like the paternal type of pluteus than did the other. This experiment was devised in order to test whether the amount of protoplasm of the egg, as compared with the possible importation of protoplasm by the sperm, is the factor involved in the maternal character of the hybrid from giant eggs as contrasted with the hybrid from normal eggs. Now in the fragment the sperm must import the normal amount of its own cytoplasm (if it does import any cytoplasm at all), while in the ½ blastomere this postulated cytoplasm has been distributed as in the normal egg. The embryo from the fragment is no more paternal than the embryo from the blastomere. The question may be asked why was not this result equally well shown by a comparison between the hybrid from a normal egg with that from a fragment of the normal egg. The answer is that the small embryos from fragments often have a less well-developed skeleton, hence they might appear more like the paternal type which is also the simpler type in this particular case. This objection is met, however, by the experiment as planned and carried out by Boveri.

The same problem comes up again in connection with Herbst's results from cross-fertilized eggs of Sphaerechinus whose development had been already started by chemical means. These eggs (normal in size) if fertilized by sperm of Strongylocentrotus give rise to plutei that are more like the maternal type than like hybrids from normal eggs. It has been shown by Herbst ('06, '07) and by Kinderer ('14) that these treated eggs have doubled the number of their chromosomes before fertilization. There are twice as many

egg-chromosomes as sperm chromosomes with the result that the influence of the maternal chromosome is stronger than when the two kinds of chromosomes are equal in number. Since the protoplasm is the same in the two cases it is clear that the result is due to the chromosomes, although here a possibility is not excluded entirely, namely, that the initial stimulus given to the egg by the parthenogenetic agent is responsible for the more maternal character of the pluteus; or else as Boveri has suggested the sperm cytoplasm may have become injured, or affected by the changes that have taken place in the egg before the sperm entered, hence its less efficient participation in the characters of the hybrid.

### Double-Sized Eggs of Ascaris

The eggs of the thread worm of the horse, *Ascaris megalocephala,* have furnished interesting cases of fusion. The eggs are said to unite in some cases before, in other cases, after fertilization. Occasionally, some of these double (Figs. 4, 5, 6) eggs appear to give rise to single giant worms. Sala ('93, '95), zur Strassen ('96, '98) and Kautzsch ('13) have described the process of fusion and its subsequent results, but their accounts differ in certain important points. For instance, there is some doubt as to the time at which the union takes place. Sala suggests that some of the unions are due to incomplete separation of the oogonia in their last divisions. Such eggs with two nuclei would, he thinks, behave like normal eggs. They would be expected to form four polar bodies, two from each nucleus, and be fertilized by one sperm. They would then contain a triple set of chromosomes. The development of such eggs was, however, not followed. In other cases double or triple eggs (Fig. 3) may be produced, according to Sala, by the action of

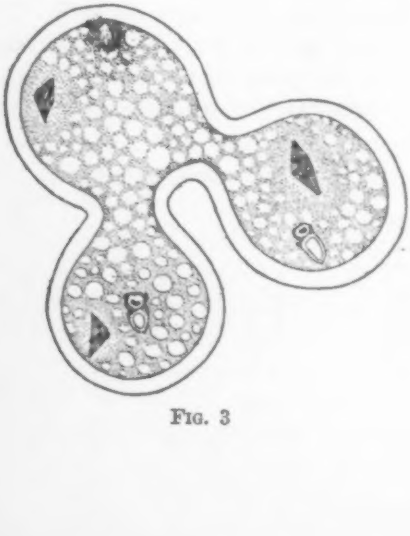


Fig. 3

cold on the eggs. The jelly formation is delayed, or, if formed, it remains soft and the eggs may stick together and even become united by bridges of protoplasm. Later, the fusion may go farther. Such unions he supposed to take place either before or just after fertilization. The number of sperms that enter the eggs is variable. The eggs later die without forming embryos.

Zur Strassen believes that the union takes place between separate eggs. He found that a low temperature might increase the number of unions, but was not the only cause of such unions. The union takes place usually between naked eggs, *i.e.*, before the jelly is formed. One sperm may enter. The polar bodies from each nucleus are extruded sometimes at opposite sides, and sometimes near together. Zur Strassen believes that eggs may also unite even after their membranes have developed. The membranes stick together, fuse, and a canal develops between the two. Through this canal the protoplasm from one egg passes and unites with the protoplasm of the other egg (Fig. 6, a). The two eggs are then supposed to flow together and unite into a single more or less spherical mass (Fig. 6, c).

Kautzsch states that a single interpretation will cover all the cases observed by Sala and by zur Strassen. He points out that if the fused eggs are arranged in the order of their stages of polar body formation, the youngest stages are always those without a membrane. In Fig. 4, a, the first two polar bodies are being given off from a double egg. One spermatozoon is present. A slightly later stage of another egg is shown in Fig. 4, b; both polar bodies are formed near together and two sperms are present. In still another egg, Fig. 4, c, the polar bodies have been given off by each nucleus, but at different poles. Only one sperm is present. In another egg, Fig. 5, a, all four polar bodies are near together in the constricted region; two sperms have entered. This egg is dumb-bell-shaped. In contrast to the last two cases the four polar bodies represented in Fig. 5, b, lie at the surface in one of the dumb-bell-shaped combinations, and the two sperm nuclei lie one

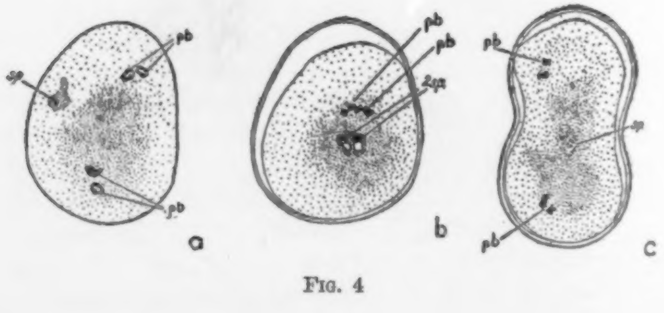



FIG. 4

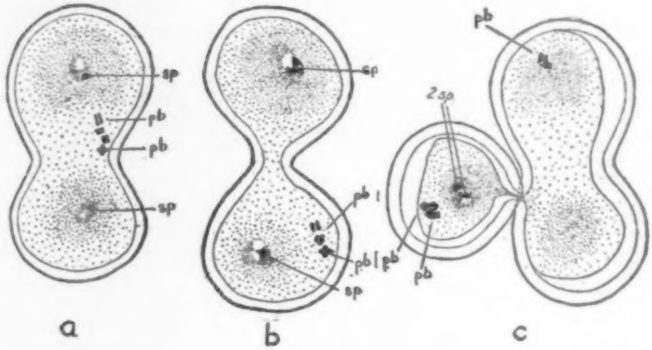



FIG. 5

in each of the rounded ends of the double egg. The union of three eggs is shown in Fig. 5, c. The polar bodies of one egg of the three lie in the dumb-bell-shaped (right hand) part, and the polar bodies of the other two eggs lie in the left hand portion that is now constricted off from the dumb-bell part. Two sperm are present in the smaller part. Kautzsch argues that in this case it is improbable that one nucleus could have passed through the narrow connection to form its polar body with the left hand egg, and that it is more probable that, after two of the fused eggs have given off together their polar bodies, a constriction appeared that forms the bridge between them. In other words, he thinks that in all these cases the eggs were at first more or less closely fused, and that after extrusion of the polar bodies and fertilization, the halves tend to round up again. This gives rise to the dumb-bell combinations seen in so many cases. Thus he reverses the order of events postulated by Sala and zur Strassen for many of the double eggs. In favor of Kautzsch's view are those cases where the polar bodies are given off near each other, for it does not seem probable that they could have been secondarily brought into this position by a union of the eggs after they had been extruded. Also in favor of his interpretation is the improbability that union could take place after the jelly and fertilization membranes had been formed. Both zur Strassen and Kautzsch agree that in later stages, after segmentation has taken place, the two eggs may sometimes come together to form a more nearly oval or spherical embryo, Fig. 6, b, c, f.

The segmentation of some of the double eggs that fused at an early stage has been followed by zur Strassen. The segmentation of many of the double eggs shows generally great irregularity arising from the presence in them of two separate egg-nuclei and one or two sperm-nuclei. When two sperm-nuclei are present, two spindles or a multipolar complex of spindles develops that leads to

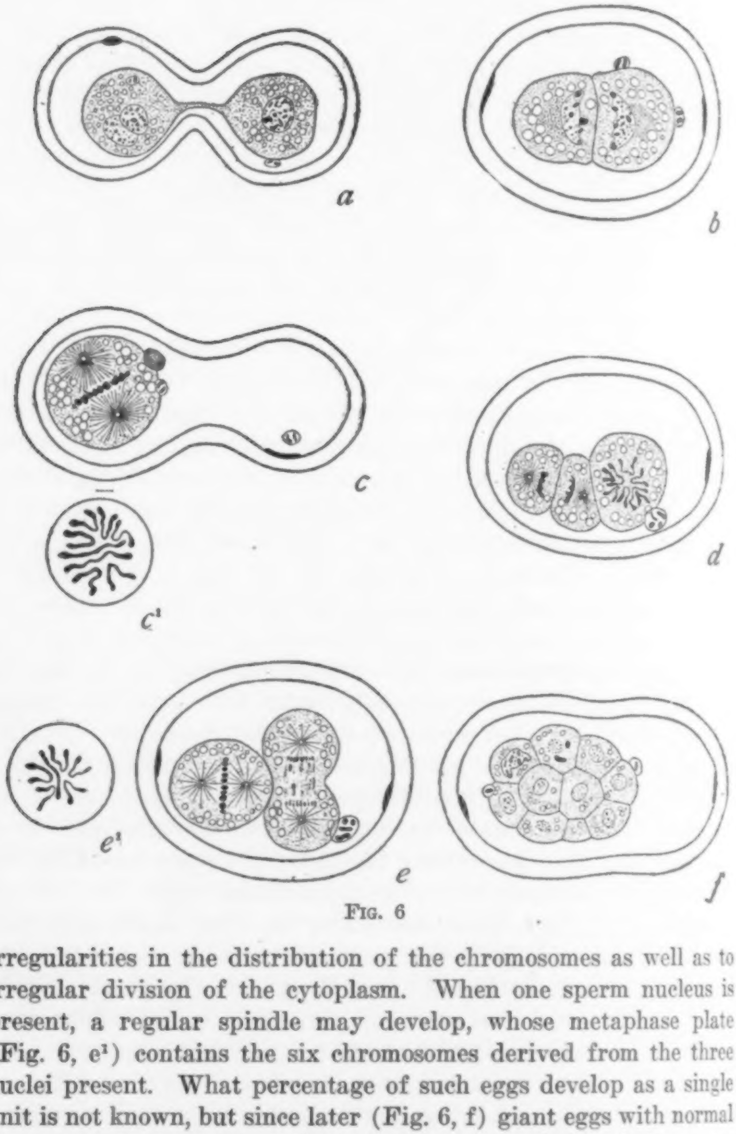



FIG. 6

irregularities in the distribution of the chromosomes as well as to irregular division of the cytoplasm. When one sperm nucleus is present, a regular spindle may develop, whose metaphase plate (Fig. 6, $e^1$) contains the six chromosomes derived from the three nuclei present. What percentage of such eggs develop as a single unit is not known, but since later (Fig. 6, f) giant eggs with normal cleavage pattern are sometimes found it is probable that on rare occasions the development proceeds in quite a normal way (Fig. 6, c, e, f). It is also possible that in other cases when two sperm have entered, a normal cleavage may take place provided a single spindle develops. The presence of 8 chromosomes in such an egg (Fig. 6, c, $c^1$ and d) is evidence that two sperm have entered. It follows that both triploid and tetraploid embryos may develop into giants. As stated above, zur Strassen records finding a giant embryo (Fig. 7,

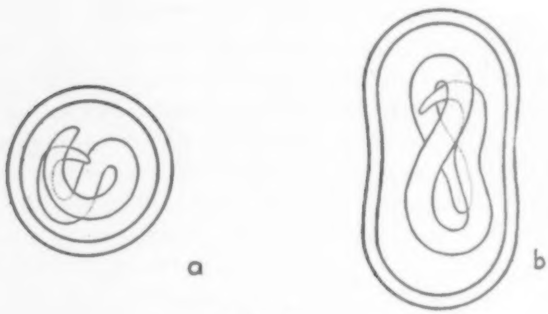


FIG. 7

b) within one egg-membrane, and the size of the double embryo as compared with the normal (7, a), as well as the size and shape of the membranes, leave no doubt as to their double origin. The results show that some of the unions at least are of such a kind that the protoplasm of two eggs and probably also the nuclei of the two eggs have united and produced a single embryo of double size.

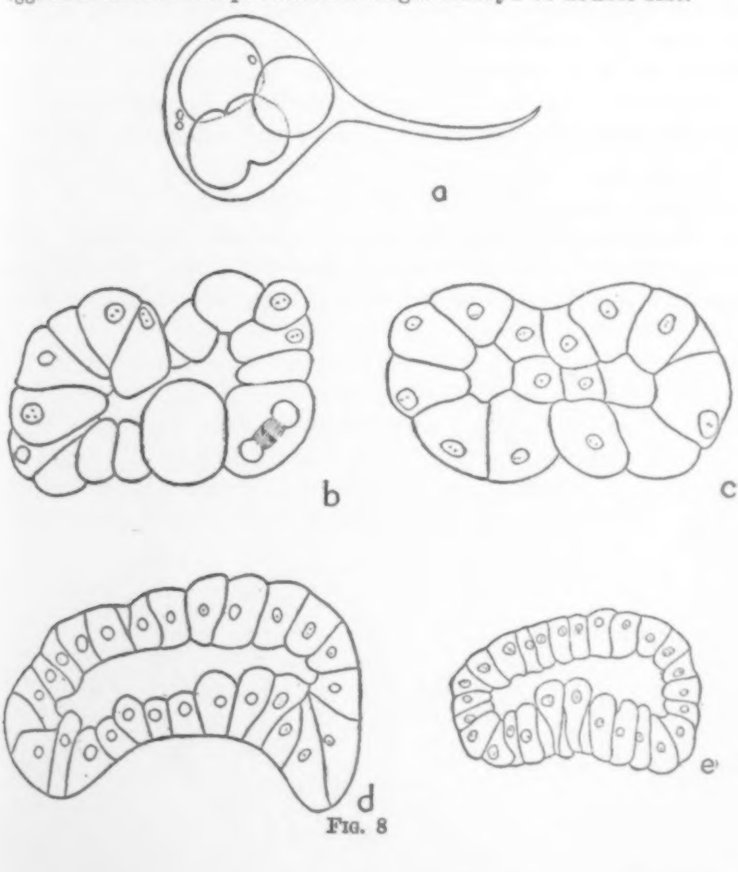


FIG. 8

### Double Eggs of Nemerteans

Giant embryos of the nemertean, *Lineus ruber*, arising from the fusion of two eggs, have been described by Jozef Nusbaum and M. Oxner ('13). The eggs are laid in cocoons, two or more eggs in each. They may fuse before cleavage (Fig. 8, a), during cleavage or in the blastula stage (Fig. 8, c). The eggs that fuse before cleavage may contain two or more nuclei, but sometimes only one. When more than two eggs fuse, the cleavage is so irregular that embryos do not develop, but when only two eggs fuse (Fig. 8, b) gastrulation (Fig. 8, d) may take place and a giant embryo may be formed. Two fused eggs may have a common blastocoel and a single archenteric invagination. These appear to give rise, at times, to single giant embryos. When partial fusion takes place between two blastulae (Fig. 8, c) each may invaginate separately. The double-headed embryos that have been found (Fig. 9, a, b) may be produced in this way.

The development of whole embryos from parts of eggs, or from isolated blastomeres, or even from pieces of blastulae in another nemertean (Cerebratulus) indicates that the blastomeres of these worms are little differentiated, or else have extensive powers of "regulation." Hence, the results of fusion of two eggs or embryos are entirely consistent with the known possibilities of these eggs.

### Giant Embryos of Triton

The union of two eggs of Triton to form a single giant embryo has recently been brought about by Mangold ('20). The eggs were removed from the jelly as they were passing into the two-cell stage.

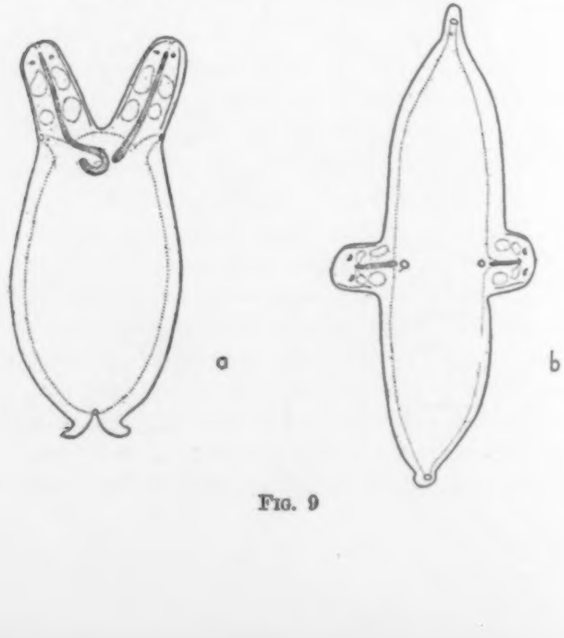



Fig. 9

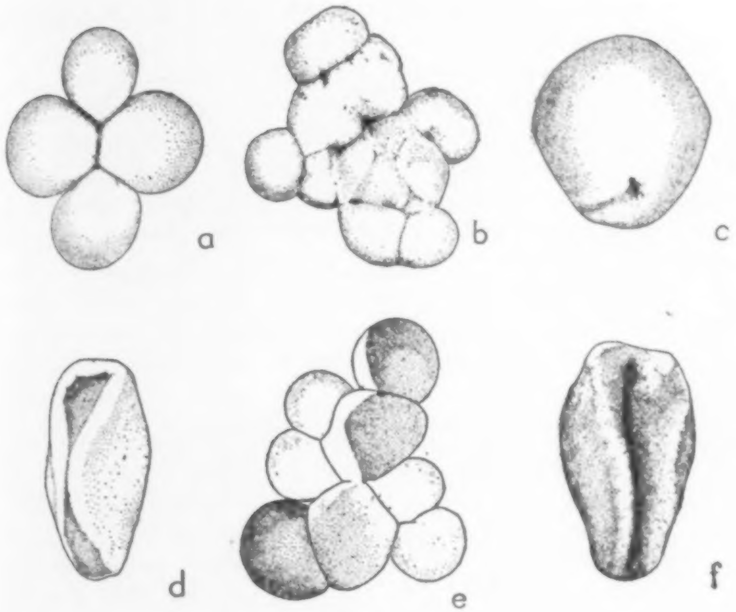



FIG. 10

In the absence of the membrane the egg flattens and the first two blastomeres at the height of the division period separate widely until they are nearly tangent to each other. One such egg is then lifted up and laid across another one in the same stage (Fig. 10, a). As soon as the four blastomeres begin to draw together they flatten against each other. This union becomes more and more intimate as the cleavages proceed (Fig. 10, b). Gastrulation takes place later (Fig. 10, c), and a single embryo may be formed (Fig. 10, d) or else two or even three embryos united together may result.

In order to understand the different possibilities involved when two eggs of Triton are brought together it is necessary to take into account the fact that the first cleavage plane is sometimes median (Fig. 11, a) and at other times frontal (*i.e.*, across the median


FIG. 11

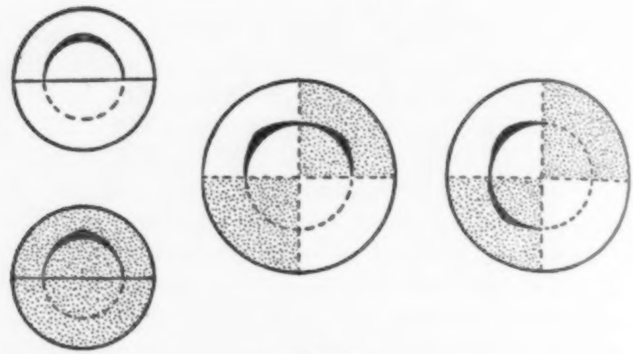


FIG. 12

plane, Fig. 11, b). By means of the following diagrams (Figs. 12, 13, 14) the possible relations of the fused eggs to each other are shown (one egg is stippled in each case). The first plane of cleavage is indicated by the straight continuous line, and the future position of the dorsal lip of the blastopore (by which the median plane is indicated) is represented by the black crescent. The two small circles to the left represent the kinds of embryos involved with respect to the first cleavage plane. The two larger circles to the right represent in each case the result of the combination of the former to produce a giant gastrula.

In Fig. 12 the first furrow in each embryo is frontal. When eggs of this sort, in the two-cell stage, are laid across each other the two possible relations of the future blastopore are represented by the two larger circles to the right. In each case the rim of the blastopore forms a continuous half circle of double size. Here the axes of the two united embryos coincide as far as possible.

In Fig. 13 the first furrow in each embryo is median. When two such eggs in the two-cell stage are laid across each other, the two possible relations are shown by the larger circles to the right. Here

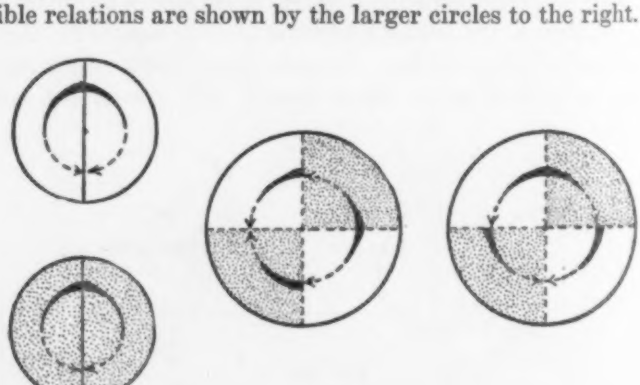


FIG. 13

there is one normal sized dorsal lip made up of halves of each embryo and two half dorsal lips in the other hemisphere. The axes of the two embryos are in the same direction, but there is only one whole dorsal lip formed by the juxtaposition of two half lips.

In Fig. 14 the first furrow in one embryo is frontal and in the other embryo median. When two such eggs in the two-cell stage are laid across each other, the two possible relations are indicated by the larger circles to the right. In the first of these a single dorsal lip is continuous at its edges with the two half lips of the other embryo. The axes of the two are nearly in the same direction. In the second, the single dorsal lip is isolated from the two half lips of the other embryo, and the axes of the two embryos are approximately reversed.

There is a very high mortality, but this is also true for single eggs that have been removed from the jelly membrane. Failure of the giants to develop is due, no doubt, in part to their exposure, but probably also in some cases to difficulties resulting from the enforced union of two eggs and the resulting maladjustment of their parts. In two cases, nevertheless, single, normal embryos of giant size were obtained. One of these came from two eggs of *Triton taeniatus* (Fig. 10, d) and the other (Fig. 10, e, f) from an egg of this species united to the egg of another species, *Triton alpestris.* It was not possible to determine the nature of the special kind of combination that gave these results, but it seems not improbable that they came from such a union as that shown in Fig. 12, or from the first union shown in Fig. 14.

Mangold also describes another monstrous embryo in which three anterior ends or heads were united into a single giant. Such an embryo is expected to arise from some of the other unions shown in the diagrams.

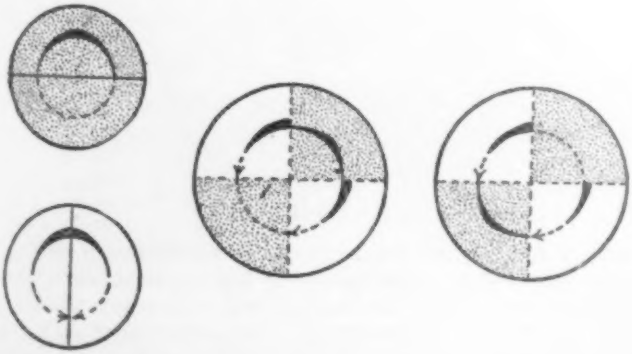


FIG. 14

These results, although somewhat meager, show, nevertheless, the possibility of forming a single embryo of Triton by the union of two eggs at the two-cell stage. They are, moreover, in accord with the results obtained from isolated blastomeres of these same eggs.[3] The outcome suggests that even although at the two-cell stage the future axes of the embryo are determined, yet an adjustment is possible if the general orientation is the same in both components. It need not be assumed that this power of adjustment is greater than that shown by the isolated ½ blastomere that have come from eggs segmenting in the median plane. The triple giant corresponds perhaps more nearly with the embryos that come from ½ blastomeres from eggs whose first division was the frontal plane.

## Double Eggs that are Not Giants

To what extent in other animals two eggs may fuse to produce single embryos is not known, but there is evidence to show that embryos may arise that are not giants but which, nevertheless, owe their origin to the fusion of two eggs; and it is also quite certain that many monstrous forms that have double structures do not arise from fused eggs. Let us consider first the latter situation. There is no evidence, for instance, that two-headed fishes, or chicks or turtles come from fused eggs. It is true there is no evidence to show that they do not arise in this way, unless the normal size of the eggs furnishes such evidence. There is in fact one case where four blastodermic areas (Fig. 15) have been found (Wetzel '00) on the same egg of a snake (Tropidonotus), and this at least suggests an earlier four-nucleated condition, but such observations are rare compared with the frequency of double embryos in other vertebrates.

Several writers have suggested that double chicks may arise from the entrance of more than one sperm into the egg, but the

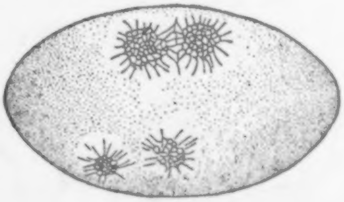


Fig. 15

[3] Mangold also produced single embryos of normal size by uniting halves of two eggs. The first two blastomeres were first separated from several eggs, then later when each of these was dividing, two of them were laid across each other. They united into a single embryo. The possibilities of combination are much the same as those described above for whole eggs, depending on the position of the first plane of division with respect to the axis of the embryo.

entrance of several sperms appears to be a normal occurrence in the hen's egg (Patterson '09). The extra sperms take no part in the later development. The not infrequent occurrence in the hen's egg of two yolks in one shell is due, as already mentioned, to the liberation of two eggs from the ovary at the same time, which, passing one behind the other down the oviduct, become enclosed in a common albumen and shell. They do not fuse and do not give rise to double monsters.[4] The multiple embryos of the armadillo have been shown to arise from a single egg (Patterson and Newman) by a sort of duplication or "budding" in a stage following cleavage. There are no grounds for assuming that the eggs have more than one nucleus, in fact, only one is figured in all the normal eggs that have been described.

The multiple embryos of certain parasitic wasps have also been traced (Bugnion ('91), Marchal ('04), Silvestri ('06), Patterson ('13, '15, '17, '18, '21)) to single eggs, each with a single nucleus. The mass of cells resulting from cleavage breaks up later into a chain of embryos.

There is one case in insects where eggs with two nuclei have been observed and where the changes that take place in them have been followed. Doncaster ('14) found in one strain of the moth Abraxas,

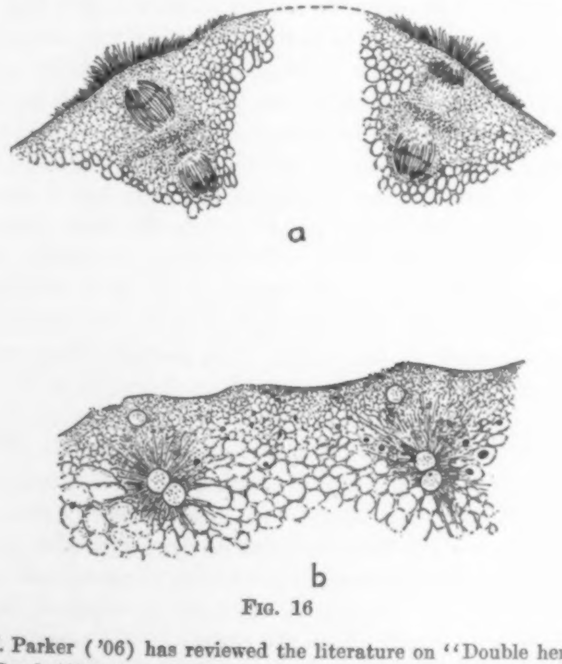



FIG. 16

[4] G. H. Parker ('06) has reviewed the literature on "Double hens' eggs." Raymond Pearl ('10) has described "A triple-yolked egg" and given references to other records of similar cases.

that certain individuals contained eggs with two nuclei (Fig. 16). Each nucleus forms its polar bodies (Fig. 16, a) and each reduced nucleus then apparently unites with a separate sperm-nucleus (Fig. 16, b). The observed entrance of more than one sperm into insect eggs, that has frequently been recorded, makes this latter occurrence not so unusual as might appear at first thought. These doubly nucleated eggs of Abraxas produce a single embryo normal in size, since the eggs themselves are not larger than normal ones. How these eggs arise is not known, but the conditions that prevail in the early germ track in insects, where a group of cells, derived from a single oogonial cell, becomes enclosed in a common follicle, would seem favorable to such a union. Furthermore, only one cell in each group usually becomes the egg while the rest remain as nurse cells. Although we do not know what conditions bring about the specialization between these apparently identical cells, it is customary to assume that the position in the group of one of the cells determines that it becomes the egg, hence it is easy to imagine that two cells failing to divide completely might reunite into one cell with two nuclei and become a double egg. Failure of such eggs to become giants may be explained by the restriction of the tube in which the egg is confined, or through which it passes during its growth stages. Possibly also in other cases the failure of such double eggs to grow to double size may be due to presence of only the normal number of nurse cells that supply a large part of the materials for growth.

Since it has been shown in moths that the female is heterozygous for a sex chromosome, it is evident that if two eggs should unite and each nucleus remain separate from the other, one nucleus after extrusion of the polar bodies might be left with the Z chromosome (the W chromosome being extruded) while the other nucleus might be left with its W chromosome (the Z being extruded). Since all the sperm are alike, *i.e.*, each carries a Z, it is obvious that a gynandromorph would arise, namely, an individual that is male on one side and female on the other. It is probable that some of the bilateral gynandromorphs that have been found in moths and butterflies may arise in this way. They are not so much double embryos, as two half embryos, one male and one female, united into one. There are still other ways in which bilateral gynandromorphs may arise and their occurrence does not necessarily mean that all gynandromorphs arise from binucleated eggs, but when a female is heterozygous for genetic factors other than those carried by the sex chromosomes, it may be possible to show by analysis that double nucleated eggs must have been the source of the mosaic individual that appears. There are, in fact, two cases of this kind described by Toyama in the silkworm moth both from the same brood. Here,

as shown by the analysis of the situation (Morgan '07, '13, '19), the two gynandromorphs must have come from a double nucleated egg. There are also a few other cases in moths where this explanation is probable. If there are no genetic factors present that make an analysis possible, the two half-individuals if of the same sex will pass for normal.

In the vinegar fly, *Drosophila melanogaster*, in which a large number of mutant races are known, and in which gynandromorphs are of frequent occurrence, there are occasional cases where the usual explanation of "elimination," in an embryonic division, of an X-chromosome from a dividing nucleus does not apply, but where the results are in full accord with the assumption of a binucleated egg (Morgan and Bridges '19, Morgan, Sturtevant and Bridges '23). At present, there is lacking the cytological evidence that is desirable before such cases can, with certainty, be referred to an egg with two nuclei, but the genetic evidence leaves little doubt as to the interpretation.

# ORIGIN AND GROWTH OF THE WEATHER SERVICE OF THE UNITED STATES, AND CINCINNATI'S PART THEREIN

By Dr. W. J. HUMPHREYS

## Meteorology in General

What is it moulds the life of man?
The weather.
What makes some black and others tan?
The weather.
What makes the Zulu live in trees,
And Congo natives dress in leaves
While others go in furs and freeze?
The weather.

This jingle has, perhaps, no claim to be recognized as poetry, but from beginning to end it is concentrated truth. The four great industries of primitive man, namely, hunting and herding, angling and agriculture, are each profoundly affected by the weather; and so, too, with scarcely an exception, are all our other and more modern industries, developed by the needs of a complex civilization. Hence, with the very dawn of his reason man of necessity became interested in the weather, nor, so long as he remains a rational being, can that interest lag.

Hence, most if not all primitive races have had their weather-wizards whose duty it was to bring rain in times of drought, or to stop the downpour and still the winds, as the needs of the people, or whims of a chief, might suggest. These miracles they sought to perform, and claimed to effect, through magic or by supplication or by some naïve mixture of the two. Nor, indeed, has civilized man yet wholly freed himself from even the crudest magic in respect to the weather, so desperate are we made and unreasoning by a withering drought, ruinous storm or devastating flood.

Next after weather-magic, in the development of meteorology, came the association of weather phases with previous occurrences; rain, for instance, with a halo the night before; or frost, for example, with bright moonlight; and so on through an interminable list, commonly expressed in the form of proverbs. Such proverbs, both those that now have the support of recorded observation and sound deduction, and also that much larger class that came from mere fancy, were among the earliest sayings to be conserved in manuscript for the benefit of future generations. They have come

to us in the Bible, in the Vedas and on cuneiform tablets. Many of them were compiled by Hesiod, we do not know exactly when, but not far from the time of Tutankhamen's youth, some 3,000 years ago. In the third century before Christ the Greek poet Aratus compiled all the weather lore of this kind known to him in his famous *Prognostica*. And the end is not yet, for this good work still goes on.

Very early, too, our knowledge of heat and cold, rain and shine, and other important weather phenomena, began forming into a more or less definite science, in the sense of being classified and explained. No one knows when nor by whom this scientific phase of meteorology was begun, but we do know that by the time of the fourth century before Christ it had grown to such magnitude that as assembled by Aristotle it made quite a treatise. We also know that for two thousand years this treatise by Aristotle was the most important of all works on meteorology, and that because of its great authority it eventually became rather a hindrance than an aid to an understanding of weather phenomena. Its logic is, of course, inductive, by which cause and effect are inferred according to association and order of occurrence. But induction is no longer sufficient in any of the physical sciences, however statistically supported the conclusions may be. We now also demand abundant deduction, or the rational prediction of effects from assumed causes. Hence, Aristotle's treatise marked only a phase, though an extremely important and long-enduring one, in the science of meteorology.

Obviously, no natural science, in the sense of classified phenomena and their logical explanation, can grow, or even come into existence without both a mass of observations and a body of logical reasoning thereon. Furthermore, in the realm of the physical sciences, of which meteorology is a member, the more accurately quantitative these observations are the more rigid and reliable the reasoning can be. Therefore, the next great advance after the work of Aristotle was the invention and use of quantitative weather instruments, especially the thermometer, generally attributed to Galileo, who is believed to have constructed the first closed instrument for observing differences in temperature in 1612, and the barometer devised and constructed by Torricelli in 1643. Crude rain gauges and wind vanes had been in use from time to time since about a hundred years before the beginning of the Christian era, but it was not until after the invention of the thermometer and the barometer, that is, not until the middle of the seventeenth century, that it was even possible to obtain numerically comparative values of either temperature or pressure, the basic factors of meteorology. Of course none knew better than even the savage when the air was

comfortably warm, nor realized more fully that at times it became bitterly cold. But such terms as hot, warm, mild, cool and cold covered the whole thermometric scale of even such great rulers as Charlemagne and William the Conqueror; such literary celebrities as Dante and Chaucer; such explorers as Columbus and Hudson; or such penetrating scientists as Leonardo da Vinci and Roger Bacon. Nor had any of them the slightest idea that the pressure of the atmosphere varied from time to time and place to place. They could not tell what made the winds to blow nor why storms came and went.

With the invention of the thermometer, however, the barometer, and certain other instruments, it immediately became possible to collect quantitative values of every important weather element, both at stated intervals by eye readings, and continuously by means of automatic recording devices. And such values were promptly collected at many places. Thus, as early as 1664, or only a few years after the invention of the barometer, Dr. Beal discovered the double diurnal variation of the atmospheric pressure, an interesting, though generally inconspicuous, phenomenon that even yet commands the attention of observer and philosopher alike. Since that time—about the middle of the seventeenth century—we have collected meteorological data in increasing abundance and with ever closer attention to details from many parts of the world. The first organized body of observers, supplied with like instruments and making similar records, was formed in 1654 by the Grand Duke Ferdinand II of Tuscany. These were located in Italy and adjacent countries, and continued their systematic observations until about 1667. Few, however, of their records have been preserved. During the next hundred years several similar undertakings were begun in England, France and Germany. Then, in 1780, the Meteorological Society of the Palatinate was founded at Mannheim under the auspices of the Elector Karl Theodor. Standard instruments were distributed to observers scattered as follows: Fourteen in Germany, two in Austro-Hungary, two in Switzerland, four in Italy, three in France, four in Belgium and Holland, three in Russia, four in Scandinavia, one in Greenland and two in North America—at Bradford and Cambridge, Massachusetts. The detailed data obtained by these widely scattered observers down to 1792 were published in twelve large volumes.

About the middle of the nineteenth century, that is, shortly after the invention of the electric telegraph, organized meteorological services began to be established, for the purpose both of systematically recording the principal weather elements and of warning those interested of an approaching storm. The first of these ser-

vices was authorized by France in 1855 and put in operation the following year. They now are maintained by all the more progressive governments the world over. Furthermore, as the success of wireless transmission grew meteorological reports to and from vessels at sea correspondingly advanced until to-day the ocean is all but as well manned and served meteorologically as the land.

Finally, near the first of the twentieth century systematic explorations and studies of the free air up to the greatest attainable heights began to be made, and are now being made in greater volume than ever before.

Whenever, now, for any reason whatever, we wish to know what the climate of any particular country, town or district is, we have only to look up its weather records—that is, if it has such records, and it always has if it belongs to a progressive part of the world. We use these accurate records of the weather of yesterday, last month or last year, in settling many a legal dispute and in answering a thousand other questions. The telegraphed reports of to-day's weather have equally varied applications. They are used by the merchant in deciding, for instance, whether he shall order a particular shipment to proceed at once or to delay for further instructions; by the citrus grower, for example, to know when the other fellow's orchard is being frosted, say, for that will immediately boost the value of his own crop; by every stock exchange; and many others for a great variety of reasons. They are also used by the forecaster in deciding what the weather is likely, in every essential, to be to-morrow and the day after—a foreknowledge of inestimable worth. Furthermore, this vast amount of quantitative data, from so many parts of the world, land, ocean and skies, furnishes both the occasion for and the proper tests of all those hypotheses and theories that together constitute rational meteorology. With so many reliable quantitative data available we are no longer confined to inductive reasoning about causes and effects in respect to weather phenomena, but can also employ deductive reasoning, by which previously unsuspected relations and phenomena are revealed. In this way meteorology has truly become a natural science—a subject concerning which our knowledge progresses by deductive reasoning and observational or experimental test.

Such, in general, has been the course of meteorology through the ages. It will be interesting next to consider how America's Weather Service, in particular, originated and into what it has grown.

### Applied Meteorology in the United States

The earliest records of weather kept in the United States, like the early records in Europe and elsewhere, were owing to individual

stimulus and enterprise. The burden they imposed on the observer was voluntarily assumed and without financial compensation. Even the recognition, or good repute—a thing every self-respecting man strives to merit—that one got from even a decennium of daily reports was mostly posthumous. We do not know in all cases just why these records were begun, but we do know that they served two useful purposes: they told with an exactness that otherwise could not have been had what the climate was of the place in question; and they furnished facts of great help for the correct settlement of questions in litigation.

The first regular record of the weather anywhere on the American continent was kept during 1644–1645 by the Rev. John Campanus at the Swedes' Fort, near Wilmington, Delaware. But Campanus soon returned to Sweden, and after that it was a long while before any one else in this country had both the desire and the patience to keep such a journal. At any rate, the next weather record in America appears to be that for 1729–1730, kept at Boston by the Hon. Paul Dudley, Chief Justice of Massachusetts. A little later, 1738–1750, Dr. John Lining kept at Charleston, South Carolina, a detailed registry of four weather elements, namely, temperature, using a Fahrenheit thermometer made and standardized in England, pressure, humidity and precipitation.

In 1739, Benjamin Franklin, during his voyage homeward from England, took full and systematic notes of the weather and of the temperature of the ocean, using a Fahrenheit thermometer.

From 1742 to 1778 Professor John Winthrop, of Harvard College, as it was then called, kept a regular set of meteorological records.

In 1743, Benjamin Franklin reached the important conclusion that a particular storm in September of that year had travelled eastward across the country. This conclusion was based on reports from many postmasters, and from the fact that an eclipse of the moon was not visible at Philadelphia, owing to the presence there of this storm, while at Boston the eclipse was seen and was over before the storm began. This was the first determination, in America at least, of the direction of travel of a storm as a whole. The rain might come with the winds from the east, but the storm itself moved towards the east.

From this time on weather records in greater or less detail have been kept almost continuously by private individuals and institutions at one or more places in the United States, as also, of course, in many other countries, including our good neighbors, Canada and Mexico. Many excellent records of this kind are still being made, and are useful, even though practically all progressive governments

are now, and for round half a century have been, keeping meteorological records on an extensive and elaborate scale.

From 1772 to 1777 Thomas Jefferson, at Monticello, Virginia, and James Madison, at Williamsburg, Virginia, made simultaneous observations of temperature, pressure, wind direction and other weather elements. These observations have the distinction of being the first made in America in accordance with the agreement that they should be simultaneous, a condition that makes them far more valuable than they otherwise would be.

The credit for taking the first official observations of the weather in this country belongs to the Army Medical Department. An order, dated May 2, 1814, makes it a duty of hospital surgeons to keep a diary of the weather; and one such journal, specifically recognizing this order, and kept at Cambridge, Massachusetts, is dated July, 1816. This order appears to have been heartily endorsed by the first Surgeon General of the Army, General Joseph Lovell, appointed in 1818. In urging the approval of the order that such diaries of the weather be kept he says: "Every physician who makes a science of his profession or arrives at eminence in it will keep a journal of this nature, as the influence of weather and climate upon diseases, especially epidemics, is perfectly well known. From the circumstances of the soldier, their effects upon diseases of the army are peculiarly interesting, as by proper management they may in a great measure be obviated. To this end every surgeon should be furnished with a good thermometer, and, in addition to a diary of the weather, should note everything relative to the topography of his station, the climate, complaints prevalent in the vicinity, etc., that may tend to discover the causes of diseases, to the promotion of health, and the improvement of medical science."

At about this same time, specifically, in 1817, organized meteorological work was begun by the government under the direction of Josiah Meigs, Commissioner-General of the Land Office, who established a system of tri-daily observations at the various land offices.

The first volume of the meteorological observations by the Army Medical Department, covering the years 1822–1825, inclusive, was published in 1826. One purpose of this publication was to stimulate the study of the then mooted question whether there is any progressive change in the climate of any portion of the country, and, if there is, how it is related to settlement and cultivation, a problem that has not yet been solved in all its details.

The second volume of these observations covered the years 1826–1830. One of the most important climatic generalizations thus obtained was the fact that, other things being equal, near large bodies of water, whether ocean, gulf or lake, the climate is more equable than it is far inland.

By this time important generalizations in meteorology also were being made, both in this country and abroad; for example, that the air cools by expansion incident to convection, announced in 1830 by James P. Espy; and that the winds blow around the centers of low-pressure storms, as explained by W. C. Redfield in 1831. These generalizations, and the scores of others that have been published during the century, nearly, since they appeared, belong to the theoretical side of meteorology, and therefore are extremely fascinating. In fact, without theory in meteorology, that is, the logical assignment of cause and effect, the growth of every weather service would be slow and uncertain. Nevertheless, weather service, not weather theory, is our present theme. Hence neither the above nor any other such laws will be further considered.

Obviously, there can be no efficient and sustained public service without public support. Hence, an important event in the origin and growth of the weather service of this country was the grant, the first of its kind in America, of $4,000 made in 1838, by the legislature of Pennsylvania to the Franklin Institute for the collection of weather information. No doubt the stimulus that secured this grant was the fine meteorological work and buoyant enthusiasm of Espy, then connected with the Franklin Institute.

The red letter year, perhaps, in the history of applied meteorology, was 1845. In that year, on the first of April, a commercial telegraph line was opened to public use. After that date any one could, and many did, see the possibility of forecasting the weather by the obvious and simple process of telegraphing ahead what was coming. However, the first person actually to begin work of this kind was Joseph Henry, then secretary of the Smithsonian Institution. In his "Program of Organization," submitted on the 8th of December, 1847, he says:

> Of late years, in our country, more additions have been made to meteorology than to any other branch of physical science. Several important generalizations have been arrived at, and definite theories proposed, which now enable us to direct our attention, with scientific precision, to such points of observation as can not fail to reward us with new and interesting results. It is proposed to organize a system of observation which shall extend as far as possible over the North American continent. The present time appears to be peculiarly auspicious for commencing an enterprise of the proposed kind. The citizens of the United States are now scattered over every part of the southern and western portions of North America, and the extended lines of the telegraph will furnish a ready means of warning the more northern and eastern observers to be on the watch for the first appearance of an advancing storm.

This recommendation was promptly adopted by the Board of Regents and money appropriated for getting it started.

By the end of 1849, 150 people, widely scattered, were taking weather observations and reporting them to the Smithsonian Insti-

tution. In this year also the telegraph lines began giving to the institution, and without charge, information in regard to the existing weather at various places.

The next obvious step was the construction of a map showing the current weather conditions over the country, and this step the institution took in 1850—only five years after the opening of the first of all telegraph lines! This map was not manifolded and distributed to the public but the single copy was mounted where it could easily be seen, and corresponding signals were displayed on the high tower of the institution. Thus were begun the first, so far as we know, systematic and organized meteorological reports by telegraph, and the construction of the first maps, for public information, showing the existing state of the weather over a large territory.

During the same time that Joseph Henry and his colleagues, Espy, Coffin and others, were doing their fine meteorological work for the land, Lieutenant Matthew Fontaine Maury, then (1844–1861) superintendent of the U. S. Naval Depot and Observatory, with the friendly cooperation of ship captains, was assiduously collecting from log books that great fund of information concerning the weather of the seas that gave us our first reliable knowledge of ocean climates. Thus was marine meteorology firmly established, a branch of weather service that to this day has grown in magnitude and increased in importance.

Maury's connection with the Naval Observatory and his activity in the collection of marine data terminated with the beginning of the Civil War. At the same time the meteorological work of the Smithsonian Institution was greatly reduced.

The center of meteorological interest and activity in this country, in respect to weather forecasting, now shifted from the capital of the nation to the Queen City of the West. On February 1, 1868, Professor Cleveland Abbe became the director of the Cincinnati Observatory, and in his inaugural report, June 30, 1868, to the Board of Control, he said:

> If the director be sustained in the general endeavor to make the observatory useful, he would propose to extend the field of activity of the observatory so as to embrace, on the one hand, scientific astronomy, meteorology and magnetism, and, on the other, the application of these sciences to geography and geodesy, to storm predictions, and to the wants of the citizen and the land surveyor.

During his directorship of the Cincinnati Observatory, Professor Abbe's active interests turned more and more to meteorology, and especially to that application of it by which warnings may be given of approaching storms. On July 29, 1868, he sent a letter to Mr. John A. Gano, president of the Cincinnati Chamber of Commerce, emphasizing the practical importance of storm warnings, and

setting out in detail a plan by which, with the aid of the telegraph and the press, he could prepare and issue such warnings.

After much consideration of this proposition, Mr. Gano requested Professor Abbe to send him a second letter that could be presented to the Cincinnati Chamber of Commerce. The requested letter was sent, also a sketch showing some of the stations from which reports would be desired, and a sample dispatch.

The happy results of this appeal are told in the following quotation from Professor Abbe's report of June 18, 1870, to the Board of Control of the Cincinnati Observatory:

> The importance of anticipating the changes in the weather, especially storms or droughts, was alluded to in my report of June, 1868. This subject having been brought by myself to the attention of the Chamber of Commerce of this city, that body, in June last, authorized me to organize a system of daily weather reports and storm predictions. Experienced observers at distant points offered their gratuitous cooperation. The Western Union Telegraph Company offered the use of their line at a nominal price. The bulletin began to be issued September 1, in manuscript form, for the special use of the Chamber of Commerce, and began to be printed a week later as an independent publication.
>
> This bulletin was supported for three months, as at first agreed on, by the Chamber of Commerce; its conduct then passed entirely into the hands of the observatory, and has thus continued until the past month. The independent publication of the bulletin was, however, discontinued, and it has, since December 1, only appeared in the morning papers. The daily compilation of this bulletin for the newspapers was undertaken two weeks ago by the Cincinnati office of the Western Union Telegraph Company, and will so continue, thus relieving the observatory of all further responsibility.
>
> In February the manager of the Cincinnati office undertook the publication of a daily weather chart, and the favor that this has met with insures its continuation in the future. The Daily Weather Bulletin and Chart are, therefore, now supported solely by the Western Union Telegraph Company, and must be considered as a very important contribution to meteorology. It would have been highly to the credit of the observatory could these publications have been maintained in its own name; but this was impossible, owing to the want of funds and assistants.

The Weather Bulletins for September, October and November, 1869, were prepared by Professor Abbe, copied on manifold paper by clerks in the Western Union Office and delivered to patrons by the messengers. These copies were known by the descriptive term "greasers."

Professor Abbe's enthusiasm over his experiments in forecasting the weather may be inferred from a letter to his father, in which he said: "I have started that which the country will not willingly let die." And his estimate was correct. The earlier meteorological work of Joseph Henry, Espy, Redfield and others in this country; the success of certain already established weather services of Europe; and Professor Abbe's demonstration that valuable forecasts, based on telegraphic reports of the weather, could be made

also in this country, left no room for doubt. The time thus had become so ripe for a National Weather Service that less than six months after the first weather bulletin of the Cincinnati Observatory was issued the federal government, through a Congressional resolution signed by the president February 9, 1870, authorized the creation of a Weather Service, and placed it under the direction of the Signal Service of the Army.

The immediate chain of events that led to this wise action on the part of Congress, an action that Maury and others had previously advocated, was as follows: Earnestly anxious to secure storm warnings for the benefit of commerce on the Great Lakes, Professor I. A. Lapham, of Milwaukee, a close student of meteorology, drew up, in 1869, a petition for support addressed to the Chicago Academy of Sciences. This petition was presented for signature to the Honorable H. E. Paine, who, instead of signing it, as at first requested, fortunately took a much broader view of the subject and insisted that the petition should go to Congress, and the weather predictions be made for the whole country and not for any small section thereof. After making the necessary changes in the original petition, and securing for it the support of several chambers of commerce and the National Board of Trade, Mr. Paine took it in charge and quickly secured its adoption by a joint resolution enacted on February 4, 1870, which, as stated above, was signed by the president on February 9, 1870.

The chief position in this newly established service, next to that of the commanding army officer, was first offered to Professor Lapham, partly, no doubt, in recognition of his invaluable services in urging the importance of storm warnings, but also because of his knowledge of meteorology and for his obvious fitness for the position. Private considerations, however, prevented Professor Lapham from accepting this offer. Professor Abbe, widely and most favorably known because of his weather forecasts at Cincinnati, was then urged to accept this responsible position and finally prevailed upon to do so. His official connection with our Weather Bureau began on January 3, 1871, and terminated, owing to ill health, on August 3, 1916, after 45 years of fruitful labor for the development of meteorology and its adaption to human needs.

The work begun by Abbe in Cincinnati on small means, but with large vision, on being adopted and supported by the national government, as was both proper and necessary, has grown amazingly; and yet, as that vision clearly discerned, the demands of the public for meteorological service have always far exceeded the capacity of personnel and equipment to supply.

## Present Activities of the U. S. Weather Bureau

The present activities of the Weather Bureau, in addition to many special investigations, and all the various duties incident to the instrumental equipment and proper maintenance of its numerous stations, may be classified roughly as follows: Observing and reporting, by telegraph and by mail; forecasting and disseminating the forecasts; assembling data for climatological needs; gathering and coordinating the weather records of ships for the development of marine meteorology; collecting and studying information bearing on the relation of weather to crops; getting and analyzing information concerning the temperature and other conditions of the upper air; reporting and forecasting river stages; measuring and studying solar radiation; measuring the kind and amount of dust in the air; and many others, generally either of smaller magnitude or not so exclusively of meteorological importance. It is impracticable with anything short of a volume, or, perhaps, volumes, to elucidate all the multifarious present-day work of the Weather Bureau. Perhaps, though, some notion of its magnitude may be gotten from the fact that in the interest of forecasting the bureau receives detailed weather reports daily, in most cases twice daily, from approximately the following numbers of stations in various parts of the world: The Far East, 12; Alaska, 12; Mexico, 20; Canada, 35; Europe, 25; West Indies, 30; the oceans (from ships), 100; United States, 200. In addition to all these, reports of the state of the upper air are received daily from six kite stations, and 40 pilot balloon stations, all in the United States.

For the detailed studies of climatology, there are used, in addition to all the above reports from the United States, similar data, sent in by mail, from about 5,000 cooperative stations, that is, stations officially equipped but tended without further cost by faithful enthusiasts—amateurs, working because the spirit of progress is in them, and deserving earnest thanks and unstinted praise.

The River and Flood Service alone gets readings of river stages from 500 different places, and rainfall from 500.

The division of Agricultural Meteorology is supplied with daily telegraphic reports from 400 stations, and written reports from 4,000.

Clearly, then (and this list is not complete), the meteorological organization of the United States has reached enormous proportions, though still far short of the development that would be necessary adequately to meet all the needs it alone could properly serve.

And what does all this contribution to science and thousand-sided aid to humanity cost the public? This practical, albeit sordid and selfish, question has a pleasing, a softly soothing answer—less than two cents per capita per year.

# DEVELOPMENT OF TRANSPORTATION BY AIR[1]

By Professor EDWARD P. WARNER
THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY

THE story of civilization is a story of constantly increasing speed and comfort of transport. Improving ease of communication has always brought in its train the possibility of preserving unity of feeling and of creating a national sentiment over larger areas than heretofore. It has contributed alike, insofar as it has appeared in any particular nation, to national safety and to the spread of national commerce. The direct economic gain from increased speed always has been, and always will be, great and unmistakable.

History offers no single instance of the long-continued neglect of a means of travel offering a great increase in speed combined with moderate economy and a reasonable degree of safety and comfort. There are, however, repeated examples of the virtual abandonment of one type of transporation in favor of another a little less comfortable, more dangerous, more costly, or all three combined, possessing only the cardinal advantage of speed. That being the case, it is not at all surprising that the airplane and airship have come into extended use for the transport of passengers, mails and goods, but it is very much to be wondered at that the United States, which has depended more than any other nation in the world upon good transportation for its economic development, should have lagged behind almost all the nations of Europe in putting aircraft to work under the auspices of private corporations, and that this country, which has firmly resisted government ownership and operation in most fields, should have been the only one in the world to depend on direct government operation of aircraft for a commercial purpose.

The one great advantage of aircraft for transport is, of course, their speed, which is far in excess of that of any surface vehicle, although the rates of travel realizable with economy are still much below those reached by racing airplanes carrying only a single pilot. A hundred miles an hour is the highest speed that can be considered as commercially practicable for the airplane at the present time, and it is better to operate at eighty to ninety where the nature of the traffic permits the slight corresponding increase

[1] Read before the American Association for the Advancement of Science—Section of Social and Economic Sciences—December 31, 1923.

of time for a journey. The airship, which will compete principally with shipping, is capable of sustained travel over distances up to 5,000 miles at about a mile a minute. There is little prospect that these speeds will be much increased, and the desirability of increasing them is open to doubt. It seems better to take aircraft as they now exist and to concentrate attention on so developing the machines and their auxiliary equipment that they will be capable of flying in all weathers and both by day and by night, with substantially perfect reliability and fair economy, rather than to seek for the production of a special form of winged projectile dependent for its operation on reasonably favorable conditions. Even the present-day commercial airplane can cut more than 50 per cent. from the train time on any long trip, and the airship will reduce the time required by the fastest ocean liner by from three fifths to three quarters on journeys over the sea.

There are, of course, some routes on which geographical conditions are peculiarly favorable to the use of the airplane, which passes over land and water, mountain and desert and forest, with indifference. The most notable example is the passage from London to the Continent, on which the traveler by air both saves a great deal of time and avoids the necessity of transshipping himself and his baggage at each shore of the Channel, to say nothing of escaping the crossing of that frequently very unpleasant body of water. Not unnaturally, it was between London and Paris that the first regular air line ran, and operation on that route has been continuous for more than four years now. The total number of passengers that have crossed the Channel by air since August 26, 1919, is more than 40,000, probably at least three times as many as have ridden on all the other air lines of the world combined in the same space of time. It is of interest to note, in view of the seeming reluctance of the American people to develop the commercial use of the airplane, that about one half of the total traffic, during the time for which statistics of nationality are available, has been made up of American tourists and business men. There has been a gradual increase in the amount of passenger business done on this particular route, and the number of passengers during a single month has run to over 2,600. The traffic has been highly seasonal, partly because of the tourist business in summer and partly because the prevalence of fogs and high winds in winter has interfered with the regularity and dependability of the service.

There are a score of other lines in Europe, most of them operating over distances much greater than that between London and Paris and most of them international in scope, for the most characteristic development of the last two years has been the suppression

of the short lines which connected two cities but had no direct share in any transportation system of continental extent, such routes having been served in some cases by two or three companies. They have been replaced by the longer routes for reasons partly economic, partly political. The saving of time by the use of the airplane becomes progressively more impressive and the usefulness of air transport to the business man becomes more and more apparent, as the length of route increases, for there is always a certain fixed time loss at the terminals, flying fields still being far from the cities which they serve in most instances. The time from New York to Boston by air, for example, including the time taken in traveling from New York to the flying field at Mineola and from the Boston airport to the center of the city, would be about three hours, a saving of only 40 per cent. over the performance of the fastest trains. From New York to Chicago, on the other hand, the total elapsed time would be about nine hours, which cuts well over half from the time of the limited train service. European airports are generally even farther out of the city than are American ones, and the use of long routes is correspondingly more important. Delays at international boundaries, too, make it possible for the airplane to show a greater relative advantage over the railroad in Europe than in this country. It is even now possible to fly from Vienna to Paris in ten hours, the best time by train being about 30, and to reduce the four days required by the Orient Express for the run from Strasbourg to Constantinople to 30 hours by airplane.

The political motive for the development of air routes is twofold. In the first place, the control of an important part of the transportation of one nation by the investors of another, working in close accord with their own government and receiving more or less direct support from it, is at once a useful, economic weapon and a means of extending national prestige and influence abroad. That its value is generally recognized by the governments of Europe has been proven by the intensity of the competition for the control of air transport in South Central Europe and in Russia, a competition to which France and Germany have been the principal parties. Second, rapid and efficient communication is an indispensable tool in the government of an empire, especially if any attempt is to be made to establish a federal system, and there is, therefore, strong incentive for the maintenance of air lines connecting the mother country with its dominions and colonies. Empires of great geographical extent arose in the distant past, but they always were purely military in nature, a single compact country sending forth armies to enslave the world. Such was Macedonia, such was Rome. The size of a self-governing unit has been limited by transportation,

and it has gradually grown from the city-state of ancient Greece and of medieval Italy, through the feudal barony offering but the most shadowy allegiance to a king, through the nation-state most characteristic of the last century, to the federal empire of world-wide extent, of which the British Commonwealth of Nations furnishes the most notable example to-day. The existing British Empire, with its present form of government and its periodic Imperial Conferences, would have been as unthinkable before the invention of the steamship as would the existing United States of America before the building of the railroads. Even to-day, the distance over which the largest empires spread is a source of strain, and aircraft, reducing as they do the time in transit of mail and passengers going home from the dominions, are eagerly welcomed by statesmen. Even where colonies, rather than self-governing dominions, are in question the airplane and airship offer an advantage in that it is easier to get the kind of man wanted for colonial service if he can keep in close touch with home affairs and spend a fair part of each annual vacation at home than it is if he must immure himself for a term of years far from home and friends and what he has learned to consider the essentials of civilization. It is such considerations as these that have led the French to run air lines out to Morocco and along the African coast from Tunis to Dakar with hardly a break, and that have led the British to point their endeavors always in the general direction of India, Australia, Egypt or South Africa. It is no mere coincidence that the three most notable long-distance cross-country flights ever made by British pilots established direct connection between the British Isles and Newfoundland, the British Isles and Cape Town, the British Isles and Port Darwin, in the Northern Territory. The latest issue of the French aerial pathfinder lists seven lines being operated under French control, and not one is confined within the boundaries of continental France. Regular schedules have been maintained during the past summer on nine routes exploited by German capital, and only one of the nine lies entirely in Germany, the others passing into Switzerland, Holland, Russia and all the Scandinavian and Baltic states. It is now possible to travel by air along a substantially direct line between almost any two of the European capitals and the whole distance of 3,000 miles from Casablanca to Moscow, with the exception of the 400-mile stretch between Toulouse and Paris, can be covered in an airplane flying on a regular route with at least tri-weekly service.

While this feverish activity in starting new companies and penetrating new countries has gone on in Europe, American development has proceeded on somewhat different lines. With no political incentive for the granting of direct government support, the use

of aircraft for passenger transport has proceeded comparatively slowly. There are several lines operating in the South during the resort season each winter, especially one between Key West and Havana, and the over-water route between Detroit and Cleveland has been regularly served by seaplanes during the past two summers, with a steadily increasing patronage. The great American achievement along the line of putting the airplane to work, however, has been the Air Mail service.

The Air Mail was started between New York and Washington during the war. As that route proved too short for full efficiency it was abandoned in the summer of 1920 and replaced by the transcontinental service, which has been functioning regularly ever since. Since every part of the route is covered in each direction on every week-day, the total distance covered during the year is a little under two million miles, the actual figure for the fiscal year 1922 having been 1,727,265. By flying under conditions which would, even during the war, have been adjudged quite impossible for airplanes, the percentage of reliability (ratio of distance flown with mail to distance scheduled) for the entire year has been raised to over 96 (for the fiscal year 1923) and the figure for the summer months has consistently been above 99. On several occasions a whole month has passed, and 150,000 miles have been covered, without a single uncompleted or seriously delayed trip. There are no recent official returns of the ratio of trips completed to trips scheduled on most of the European passenger lines, but it undoubtedly is, in all cases, somewhat inferior to the Air Mail's ratio. During the summer of 1921, for which complete statistics have been published by the British Air Ministry, the average percentage of the commercial airplanes operated by British companies was 95 on the London-Paris route and 89 between London and Amsterdam, despite the prevalence of fog over the Channel. It has been demonstrated very conclusively, both in Europe and here, although more effectively by the United States Air Mail than by any passenger line, that at least 95 per cent. of all scheduled trips can be completed on schedule time during eight months in the year, using the airplanes and other equipment now available. In strict adherence to a time-table the airplane need not fall far behind the railroad, although, to be sure, when the airplane is delayed by impossible weather or by a forced landing the delay is likely to be a long one. Such delays are becoming progressively rarer as the reliability of engines is improved and as better methods of navigation for flying in bad weather are developed. Every meteorological obstacle except fog has already been overcome, and progress is being made in conquering that.

Any further extension of commercial aerial transport facilities

must be the result either of the financial success of those lines which are operating already or of very conclusive proof that a profit can be made with some new line or some new scheme of operation. The governments of the world have so far lent direct support, for all the major and most of the minor states of Europe have offered subsidies, but that policy can not be pushed much further without excessive burden on the tax-payers. If air lines are to take a really important place among the world's means of transport they must be self-supporting.

The subsidies, helpful in some cases, have been definitely harmful in others. Since they have been given largely to encourage the aircraft industry and to build up a source of military equipment, many of the grants are hedged about by restrictions which force inefficient operation and the use of airplanes of a type quite unsuited for economical commercial use. They are having an effect not dissimilar from that of the French ship subsidy law of three decades ago, which had the effect of turning the clock backward and reversing the natural course of development by fostering sailing ships at the expense of those propelled by steam. Such a subsidy may satisfy the operators for the time being, but it offers no incentive to the search for improved methods and equipment, and it certainly does not represent the proper road to the upbuilding of a strong and self-reliant business. Furthermore, the very generosity of a subsidy may discourage efficiency and prevent an energetic search for traffic and attempt to stimulate public interest in flying. Where the state guarantees all expenses there is no reason to seek passengers, who must be cared for at the terminals with some expense and trouble and whose presence on board the aircraft involves the risk of suits for heavy damages in case of any accident, however slight. An exaggerated subsidy has the effect, also, of stimulating the creation of lines where there is no important public service to be rendered. An illustration of the extreme disproportion between some of the subsidy grants and the amount of actual business done in securing them is afforded by the case of a certain French company which, during 1921, received 6,419,000 francs in subsidies, and 529,400 francs gross from the public for the transporation of passengers, express and mail. It should be said, however, that this is not a typical case, and it is necessary to emphasize again that some of the air routes were selected for political reasons and have no economic excuse for being.

Even the most generous subsidies involve comparatively small expenditures, as national budgets now go. In 1921, for example, French air lines covered a total of 1,391,000 miles on a subsidy of 25,366,865 francs (about $1,800,000), and this year's recommended budget provided for a grant of 45,250,000 francs.

The rates on air lines at the present time under liberal subsidies obviously have but little significance, and in many instances they are fixed by the government quite without regard to the costs of operation. There are a few European countries, however, which offer only very moderate assistance in meeting running expenses, and two or three lines there, in addition to all those in America, are getting along with no subsidy at all. The British subsidy to all companies combined totals only £95,000 ($425,000), and the passenger rates on the various routes which those companies serve range from 3.8d to 6.6d (7c. to 12c.) per passenger mile, including 30 pounds of baggage for each passenger. The French, who are the beneficiaries of one of the most liberal subsidies, charge an average of 1 franc (5½c.) per passenger mile. German rates are, of course, still lower. They were equivalent to approximately 2 cents a passenger mile in the summer of 1922, but the recent gyrations of the currency make it futile to attempt the statement of any equivalent for the rate, either in marks or in gold. The German government itself has abandoned the effort, and now awards the subsidy in gasoline or the equivalent in currency at the price momentarily prevailing.

The higher of the two British rates quoted above represents about the best that can be done on a self-supporting line with at least ten passengers traveling each day in each direction and with airplanes of existing types. It has been the general conclusion of those who have analyzed the subject and prepared hypothetical balance-sheets with direct reference to American conditions that it should be possible, given that amount of traffic and operating twelve months in the year, to run at a small profit with a rate of from fifteen to eighteen cents a mile. The operations of the Air Mail have given invaluable testimony on that point, and have removed the estimates from the realm of pure theory, for very full reports of all expenditures, including departmental overhead and all salaries of officials in any way connected with the handling of the mail by air have been compiled each year. The actual cost per airplane mile has ranged from 70 cents to a little over a dollar, but in the year when the expenditures ran highest a large part of the outlay was for permanent improvements to fields and buildings. Eighty cents may be taken as a fair average. It is possible to build airplanes, using the same engine as that used in the Air Mail planes and costing no more to maintain, which will carry nine passengers besides the pilot. If a 78 per cent. load, or seven passengers per trip, be assumed, the per capita cost would be 11.4 cents a mile, and a rate of 16 cents leaves a 30 per cent. margin for selling expense and profit.

To reduce this cost, either the flying or the ground expenses must be reduced. While there are considerable possibilities in the first direction, possibilities in the production of airplanes of increased efficiency which will be realized with the further development of design, the greatest saving at the moment can be made on the ground. Air transport at present is being conducted on far too small a scale to be efficient. With one airplane a day in each direction, and an office force and staff of mechanics at each terminal and at every intermediate stop, the overhead becomes enormous. With seventy passengers each day, instead of seven, the rate should come down to not more than ten cents a passenger mile, or about double the charge on extra-fare trains in compensation for more than doubled speed. There should be little difficulty in getting business at that figure. The actual flying expenses of the Air Mail (fuel and oil, pilots' salaries, repairs to airplanes, etc.) so far as they can be segregated from other items are about 40 cents an airplane mile.

Only through large-scale operation, too, can one of the greatest advantages of air transport be realized. All high speeds in the past have been reached by the use of large units of conveyance. The fastest ships are the largest, and the fastest trains are the heaviest and longest of passenger trains. Only in the airplane can speed be combined with small size, and only the airplane permits of operation of high-speed vehicles at very short intervals. The present demand only justifies the operation of two extra-fare trains a day between Boston and New York, but one fourth the number of passengers carried by those two trains would fill a nine-passenger airplane every 40 minutes during the business day. The gain in usefulness to the business man would be inestimable, but it is essential, if the American people are to take advantage of the possibilities of aircraft, that the development of air transport be taken up and fostered by interests of sufficient financial strength and far-sightedness to provide the capital needed to start operations at once on a scale large enough so that the airplane service will always be available within a few minutes of any desired time and so that it may present a real saving over the train. There are routes in America where there is a clear need for such a service to-day.

The airplane alone has furnished the data for this brief treatment of the economics of air transport, and there has been but little experience in the commercial utilization of any other type of aircraft, but the airplane will have to divide the field in future with the lighter-than-aircraft, the airship. Technical limitations are such that it is not now possible to operate the airplane for more than 300 miles non-stop with reasonable economy and commercial efficiency, nor does it seem likely that that limiting distance will be

very much increased in the future. The airship, on the other hand, can easily make voyages of three or four thousand miles with a good commercial load, and it will have to form the chief reliance for trans-oceanic aerial travel. It is to airships that the British are turning for the projected London-Australia service.

The load carried by the airship for every unit of power is considerably greater than the corresponding figure for the airplane, and operation is correspondingly cheaper, despite the added item of replacements of gas. Although there is little experience on which to base an estimate, and will not be, in America at least, until the Navy publishes the figures which are to be obtained with Shenandoah and the ZR-3, it is probable that a passenger rate of seven cents a passenger mile can be maintained. With an allowance of twenty dollars a day for meals, service and other "hotel expenses," the cost of a two-day trans-Atlantic passage would hardly exceed the first-class charge on a six-day boat, although it must be admitted that the accommodations would at present be somewhat inferior in luxury to those on the liner.

Whether the airplane or airship is under consideration, there is one form of assistance which must be secured from the government, and which has no relation to a subsidy. It is impossible to operate without landing fields, maps; it is impossible to fly regularly and safely without meteorological reports broadcasted by radio, and other aids to navigation, generally grouped with the landing fields under the title of "ground organization." The provision of these things, which will be available for all who fly and will be used by all, is solely a government function, as surely as are the establishment and maintenance of lighthouses along the coast, the dredging of harbors and the publication of charts. To propose that some company should assume the whole burden, even on a single route, can only be described as preposterous, and there is unlikely to be any regular commercial flying on any overland route which has not had the benefit of some governmental development. The entire absence of overland air transport in America to-day is chargeable primarily to the unwillingness of Congress to give attention to the needs of commercial aeronautics and to the economic possibilities of aircraft.

Important as is ground organization for all regular commercial operation, it is the absolute condition of existence of night flying. Obviously, aircraft will not render the full benefits of which they are capable until they can give a twenty-four hour service, with over-night travel from New York to St. Louis or Chicago to New Orleans. So far as the airplane and its pilot are concerned, there is nothing to hinder night flying at this moment. The Air Mail

has proved conclusively by actual demonstration that an airplane can be flown by night with substantially the same safety and certainty as during the day, but only on condition that aids to navigation are provided. Although the airplane itself carries lights which are of great assistance, and avoid the necessity of floodlighting the whole area of the landing field to a uniform brilliance when a landing is to be made, it is still necessary to place lights around the edge of the field to define its boundaries and to warn the approaching pilot of any local obstacles such as buildings or flagpoles. Such provision must be made not only for the regular terminals and fueling stations but also for intermediate emergency fields permitting landings in case of engine trouble.

Night-flying organization must include the marking of the route as well as of the fields. Aerial beacons must be established every twenty or thirty miles, each throwing aloft a beam of such intensity that the pilot can rely on having at least one light always in sight. The provision of such lights bears a perfect analogy to the safeguarding of our coasts by lighthouses and buoys, and the government which has long provided for sea and lake traffic will have to do as much for the voyagers of the air. One experimental route, that between Chicago and Cheyenne, has already been equipped, and it was over that route that the Air Mail's experiment of last August, during which a continuous day-and-night service from coast to coast was maintained for a week, was made. The experiment succeeded in every detail, but the night service could not be continued because of lack of funds.

Experiments in night flying during the past year have served to concentrate attention on the use of aircraft for express service. Airplane design has not yet progressed quite to the point of offering comfortable sleeping accommodations. That development can be foreseen for a future not very distant, but for the moment it is easier to use existing types for the carriage of cargo when all-night travel is involved. Express matter can be carried between New York and Chicago at sixty cents a pound, with collection in one city late in the afternoon and delivery in the other early the next morning. The rate is high, compared with that by train, but the saving of time would more than justify the difference of cost for certain classes of goods which now go by train, and if 2 per cent. of the express traffic which now travels by rail between the two cities were to be diverted to the air routes it would require the operation of half a dozen 400-horsepower airplanes in each direction every night. Already officials of one of the great express companies have declared themselves willing to cooperate in securing business for the airplanes and in handling collections and deliveries through their existing organization.

In view of the probability of an early development of aerial express service in America, it is of interest to observe the trend in that same direction in Europe. Although the use of the air lines by passengers has, as already shown, been steadily increasing, their use for mail and express has in general advanced even more rapidly, especially on those routes which seem likely to make but little appeal to passengers. The passenger business between France and Morocco, for example, is almost negligible, but the weight of goods carried has rapidly increased until, in 1921, it totalled about 35 tons, for the whole year, while in June and July the present year the mail and express for the two months amounted to over 20 tons. In April and May, 1922, the total weight of the express and mail matter handled by the French air lines running to and from Paris was 198,700 pounds, while the corresponding figure in 1923 was 349,000 pounds, an increase of 76 per cent. Between the same two periods the passenger traffic on the same lines increased only 13 per cent. The absolute figures are still small for all classes of business, but the upward trend of the curve is unmistakable.

That last statement, indeed, epitomizes the situation. The public has been slow to accept air transport, but there is ample evidence to show that progress is being made, and that the airplane is gradually coming to be regarded as a commonplace vehicle for the conveyance of ordinary people and their goods in the ordinary course of business. We shall have occasion for profound regret if America, the original home of the airplane and a country always known for its quickness to adopt the improved method in transport and in industry, continues too long to lag behind the other nations of the world in the exploitation by private enterprise of the commercial possibilities of aircraft.

# PASTURE

By SPENCER TROTTER
SWARTHMORE COLLEGE

## I

PASTURE—and the seeking after fresh pastures—has always been a fundamental motive in man's history. In the mist that shrouds that ancient world of men, which we of to-day call the "prehistoric," certain tribes were wandering with their flocks and herds out of some eastern land—Egypt, Moab, the "fertile crescent" of Mesopotamia and Palestine, the plains of Trans-Caspia—northward and westward into Europe, moving slowly, century after century, always in search of fresh pasture—in the dawn of history. These ancient shepherds and herdsmen were a neolithic folk, using stone implements, polished and ground to fine edges and points, and in later times working out the same patterns in copper and still later in bronze, but all the while pastoral by nature. The most familiar record that has come down to us of the life of some of these tribes is that depicted in the early Hebrew Scriptures, where the wars were essentially wars for fresher and more extensive pastures. The life of these peoples revolved about their flocks and herds and their language was filled with pastoral allusions—the lamb as a burnt offering on an altar of stones, the blood of bulls and of goats, the ashes of a heifer, a land flowing with milk and honey, green pastures and still waters—all such expressions could only have come out of the life of shepherds and herdsmen.

In their far-and-wide wanderings the vanguard of these neolithic and bronze men encountered a ruder hunting folk, men who hunted the aurochs, the red deer and the horse, who still followed the retreating herds of reindeer to the north and killed the last remaining mammoths with their rough stone spears and javelins. The short space of written tradition which we regard as history is but as yesterday compared with the thousands of years during which the pastoral peoples spread over Europe from the east in successive waves of migration, seeking—always seeking—new pasture. In Asia Minor their herds of sheep and goats are thought by some writers to have nibbled and tramped down the grass to its very roots and to have thus, in a land of little rain, prepared the way for the encroaching sands that made deserts of once fertile regions and forced the grazing herds to move towards lands of more abundant grass. In those prehistoric millennia the pastoral nomads in Europe no doubt long dwelt in the midst of the hunting peoples,

the remnant of the old "reindeer men" of paleolithic culture, as settlers in a wild land have always dwelt surrounded by savages—in North America and in Africa. The once glacial meadows of Central Europe were covered with lush grass, with buttercups, daisies and other wild flowers—rich pastures for the increasing flocks and herds of the invaders and, as has always been the case in every land and age, the wild animals dwindled and the primitive hunting life gradually disappeared as a dominant feature.

The question of pasture has always been of vital concern in the life of a hunting people as well as in that of the herdsmen. A fundamental factor in the evolution of the human type, wherein it broke away from some anthropoid stock of entirely vegetable feeders, was the acquirement of a more general diet into which the flesh of animals largely entered. At first a feeder on carrion, man gradually took to killing the large herbivorous beasts that roamed in herds over the land, and the wanderings of these game herds in their search for new pastures governed the movements of the primitive hunting population. Wherever the wild herds pastured, there were hunters on their flanks, wandering far and wide as the game shifted from one territory to another, and the earliest records of human history are in the rough spear heads and stone axes of the hunter's craft. In whatever area of the earth man originated he spread from this early cradle of the race with the grass feeders, slowly widening his range until every portion of the great continental land masses was occupied. America was thus undoubtedly peopled by some primitive Asiatic group that wandered with the deer and the bison herds, and quite likely, too, with early elephants like the mastodons, across the Behring land-bridge. It has ever been the lure of pasture in human history.

The influence of pasture began long before man's advent upon the scene. Had it not been in the scheme of plant evolution that the grasses came into existence there might have been a very different creature than man as we now know him. Not until comparatively late geological time did the grass type appear and also flowering plants like the vetches and clover, the crowfoots, heaths and compositae, which with the grasses cover vast areas of the earth. Without this abundant food-supply, rank and deep and forever renewed, the great mammalian fauna could not have come into existence. In the scheme of evolution man thus appears as one of the results of pasture.

## II

Undoubtedly the most far-reaching achievement in human history, next to the discovery of how to kindle and keep a fire, was the domestication of certain species of grazing animals. In what way

this was accomplished is entirely a matter of conjecture, for it is lost, as the beginnings of so many human faculties are lost, in the dim twilight of the ancient world. The outstanding feature of this achievement of the primitive genius is the fact that no other wild stocks have ever been domesticated within the ken of recorded history, not a glimmer of tradition even has been vouchsafed to men of later times as to how it came about. It must be regarded as a trait of the primitive mind. If we choose to speculate at all on these origins and the absence of any later effort to tame and use other wild stocks we might possibly view their problem from one or more angles; either it came about by the capture and raising of the young of certain species and the subsequent discovery of useful qualities in a more or less accidental way, or, by a process of reasoning, primitive men deliberately set about to capture and tame individuals of several species with the view to making use of them in advancing human welfare, and that domestication once established there was no further need for any new additions. There is archeological evidence to show that domestication of some animals came to an end and was never again attempted. In certain Egyptian reliefs several kinds of antelopes are depicted as being "stall-fed," and the species are clearly recognizable, all natives of Northern Africa. On the tomb of Mereruka at Sakhara (twenty-seventh century B. C.) these are shown as tied to their mangers, and likewise on the tomb of Kegemni of the same century mention is made of "stables of the plateau antelopes." What use these animals were put to we have no means of knowing.

There is no doubt but that pastoral life developed out of a hunting life, the animals domesticated having been originally hunted for food. All we know is that the wild ancestors of most of our domestic stock have absolutely disappeared, merged probably in their entirely domesticated descendants. Some light is thrown on this by archeological researches in Egypt where a primitive hunting people during late paleolithic times (men who were contemporaneous or even earlier than the cave men of glacial Europe) appear to have begun the process of domestication of certain bovine species and also of the wild steppe ass of Nubia. Predynastic reliefs of a date not later than the fourth millennium B. C. show cattle, sheep and donkeys, species which even at that time must have been long under domestication. The antiquity of domestication in Egypt is further attested by reliefs showing a milking scene and also of a hornless breed of cattle (tomb of Ti at Sakkara, 28th century B. C.). Now the abstraction of milk from an animal by the hand of man would surely argue for a very long period of domestication. Several wild stocks may have contributed to the ancestry of our

pasture animals. From the beginning they have been an integral part of man's existence; without them we can not conceive of human history as an advance from lower to higher estates. Their reaction on agriculture was of the first importance. Egypt, that storehouse of antiquity, reveals in its reliefs how the primitive hoe was converted into the ox-drawn plow (plowing scene from tomb relief, 26th–27th century B. C. in the Louvre, Paris).

The relation between agriculture and the pastoral life has a far deeper significance than the effect on implements and methods. It lies at the very roots of the social state. In primitive times woman was the agriculturist and it was by her labor that the earth was first made to yield its increase. About her rude home she scratched the soil, sowed the gathered seeds of various wild plants and thus grew the earliest kitchen garden which insured food for herself and her children. It was she who first most likely domesticated the dog by the care and nurture of the puppies of some wild wolf-like animal that from time to time were brought into the home by her men, even suckling them, as has been observed among some tribes in Australia and America and as related in certain aboriginal tales where a dog is spoken of as a foster-brother. When it came to taming the great beasts of the pasture, that was a man's job and was the foundation of the patriarchal stage of society. In primitive times the social state was a matriarchate or mother-right, but as the pastoral life became more and more a dominating feature in the life of the early Asiatic and North African peoples and hunting became supplementary, pursued against predatory beasts and for sport, man acquired an increasing interest in agriculture and slowly took over much of the woman's share of primitive husbandry. In doing this he also took over the woman herself as a possession and she became part of a social state in which the family was a unit governed and controlled by the man. Traditional history opens with this patriarchal stage, the nomadic shepherd and herdsman being a chief at the head of some clan of several families united by blood, continually clashing with neighboring clans for pasture rights. With this power vested in the man polygamy was a natural consequence, for the more children a man had to tend the sheep and cattle, and sons to aid in the primitive warfare, the larger would be his herds and flocks and the wealthier and more powerful he became. The abundance of milk furnished by the herds was a further cause of family increase, insuring a steady and sufficient food-supply for young children. In nomadic life a man's household was maintained and grew in proportion as he was able to extend his pasture. The word "daughter" bears witness to a primitive division of labor in the patriarchal family—"a milker"—the eternal

goddess of the herds in every age and generation, milking her cattle in byres and on the edge of pastures, at daybreak and in evening twilight, since the dawn of history.

## III

Walter Pater somewhere speaks of "that strange mystical sense of a life in natural things, and of man's life as a part of nature, drawing strength and color and character from local influences, from the hills and streams and natural sights and sounds."[1] In nothing is this more real than in things pastoral. A love of pastures and the life of pastures is our natural inheritance. The spell of wild lands may hold us for a time, but the call of the pasture is ever in our blood and we come back to it as a child to its mother. The smell of grass is as the very breath of our nostrils. In pastures, as in gardens, the soul of man finds delight. It may be among the hills where the pastures slope up to the sky-line—rock-strewn uplands with their stone-wall boundaries and the trilling of vesper sparrows heard in lulls of the wind, or in deep-grassed meadows through which some quiet stream meanders. Izaak Walton was a lover of such meadows and "The Compleat Angler" is as much a call to pastures as it is to fishing. Listen to this opening verse of his "Angler's Wish":

I in these flowery meads would be:
These crystal streams should solace me;
To whose harmonious bubbling noise
I with my angle would rejoice . . .

And again in "The Milkmaid's Song," by Christopher Marlowe, which Walton has Maudlin sing—

Come live with me, and be my love,
And we will all the pleasures prove
That valleys, groves, or hills, or field,
Or woods and steepy mountains yield;

Where we will sit upon the rocks,
And see the shepherds feed our flocks
By shallow rivers, to whose falls
Melodious birds sing madrigals.

In the high Alps there are pastures that creep up to snowfields and the edges of glaciers, friendly and home-like places among the stark peaks. And there are the great unfenced pastures of the world—the African veld, the pampas of Argentina, the prairies of

[1] Preface to "Studies in the History of the Renaissance."

North America, the Central Asiatic steppes, where the herds of man wander half wild and often in sight of strange beasts. Our settlers on the prairies fifty years ago and the early Boer farmers in South Africa were witness to this stirring contact with the wild life. Gordon-Cumming, when living in Cape Colony in the first half of the last century, relates how one evening when his man was bringing in the cows to be milked four lions appeared on the edge of the pasture—an exciting event but not an uncommon one on South African farms at that time and even to-day in such regions as British East Africa and Northern Rhodesia.

This touch of wild life is in every pasture. The wary fox crosses the open stretches of grass and will sometimes stop to dig out a field mouse. On mountain farms in old settled districts a bear occasionally shows himself along the forest border of fields where cattle and sheep are feeding. Old Peter Little once told me, as we stood by his house in a hemlock clearing up in the Pennsylvania mountains, that when he was a young man wolves killed the stock on that same mountain farm, and he swept his hand towards the edge of the woods where he had seen them skulking in the dusk. The woodchuck still makes his home in our pastures, scuttling into his burrow at sight of us, and the skunk and weasel peer out from bordering thickets. Since the country has become domesticated many kinds of birds frequent the grasslands—the call of the bobwhite in early summer is the very spirit of old pastures, the meadow lark flushed from grassy cover, the killdeer wheeling about with its wild cry, the strange cowbird walking among the cattle, the scattering flocks of grackles and redwings in autumn and their return in early spring, the crows foraging in winter—all have the pasture as a background and help to cast that spell, that mysterious feeling of delight, of release from the sordid things of life which the true lover of the countryside knows so well. And the same is so with the pasture blooms—the white patches of spring beauties and bluets, the later gold of buttercups, the meadow-sweet and the daisy crop, the blue iris in moist places, lavender-topped grasses, hellebore and St. John's wort, and the rank pasture growth that comes at the end of summer—iron-weed and boneset, the milkweeds and thistles, the lobelias and gentians, and along old fences and stone walls the autumn-blooming asters and goldenrods. These later blooms of the pasture exhale a peculiar fragrance, heavy and medicinal, that stirs up something in the subconscious that is indefinable but altogether delightful. It is a part of that strange magic which reveals the kinship of man with natural things—with what William Sharp calls the "green smell of grass," with freshly turned plowland, and the smell of woods.

Certain races are countrymen at heart, even though the vast majority are doomed to dwell in cities. The old American type, the stock that sprang from the loins of western Europeans in the seventeenth century, has this fortunate heritage—the love of land and of open country. Agriculture first induced permanent settlement in certain fertile river valleys, and centralization in cities was a natural consequence, for man has ever been a buyer and seller of commodities. But the most ancient coin carried the stamp of an ox on its face, and this meant pasture. The grass and grain crops have always been grown to feed the herds, to carry them over periods of shortage when the fresh grass was scanty. Animal flesh and milk have largely made man what he is, but it is the pasture, after all, that enters into his being—by his very nature he is part of it. It is this that is the real life of man—this ancient thing foreordained to bring about the human type. "All flesh is grass" has thus a deeper meaning than that of a transient existence which it was intended to symbolize. It is man himself, and not only his bodily organization but his mind also is an expression of this fundamental relation. It comes to many of us as an interest in the animal life of the world of which men are but a part; to some it is a love of woods and fields, of old farm holdings on hillsides and in valleys, of wide prairie lands and wild places of the earth. The artist and the poet tell of this feeling on canvas and the printed page. In every man who is alive to these influences there is established in his soul something that is primitive and which endures—some sight or smell or sound that recalls the agelong intimate relation of his race with things pastoral.

# SOME HISTORICAL ASPECTS OF EXPERIMENTAL PHYSIOLOGY

By Dr. GEORGE R. COWGILL
YALE UNIVERSITY

As is well known by students of the history of science, the extensive use of the experimental method as an agent with which to find new truths began during the early decades of the nineteenth century. To be sure, definite experiments were resorted to by many scientists before this time, notably Galileo, who experimented with the pendulum, Newton, who studied the dispersion of light by a glass prism, and Harvey, whose classical investigations led to the discovery of the circulation of the blood. Such instances, however, were unusual rather than common and, if viewed from a sociological standpoint, may be regarded rather as the expression of that surging spirit of unrest suffering repression, which Rousseau has so aptly expressed in the words, "man is born free and everywhere he is in chains." The number of men in any one century before the nineteenth who thus rebelled against the sway of ignorance and strove to overturn its rule was small; compared with the number of those who thus labored during the past one hundred years, it seems to become very insignificant indeed.

Such a condition is not difficult to understand, however, when the social fabric of those earlier centuries is considered. Reverence for authority was to be found in the field of medicine and biology as well as in the domain of theology. This fact alone should teach us how remarkable is the hold and influence upon all society of such intangible things as the psychological factors in life, "modes of thought," "social complexes," "mores," or whatever one chooses to call them. For nearly fifteen hundred years the physiological concepts that were taught were those of the great Roman physician Galen. Vesalius in 1542 was the first to protest against Galenic authority in anatomy when that authority failed to agree with anatomical truth as revealed to the naked eye by skillful dissection; but in spite of the emphasis which this gifted man placed upon accurate observation of nature alone, so few were the workers who used the scientific method even moderately that the world had to wait for nearly seventy years before such an individual appeared in the person of Harvey, who solved by definite and carefully planned experiments the great mystery of the passage of the blood from the right to the left heart.

Previous to the nineteenth century physiology as an independent science did not exist. During the eighteenth century and earlier this subject was taught as a phase of anatomy, the function of organs being discussed by the teacher at the same time that the structure was being studied.

Stenson, whose "Observationes Anatomicae" was published in 1662, referred to the physiological studies, of which he was cognizant, as *anatomical experiments.* In an appeal on behalf of the practical value of science he said:

> It may be shewn abundantly elsewhere how much medical practice owes to the anatomical experiments of this age, even if it were only for this that they have exposed the numerous errors which occur in the explanation of the causes of disease and at the same time shewn the reasons which have governed the application of remedies to be in most cases erroneous. To those who decry the value of science I would give as an answer this demand that they should ask their own consciences and see what solid basis there is for all those dogmas which they pronounce with such bold ease when they explain the symptoms of apoplexy, paralysis, convulsions, prostration of strength, syncope, and other diseases affecting animal movements, on what foundation they rest when they apply remedies for removing these evils, with the result that they do away not with the paralysis, not with the convulsion, but with the paralytic or the convulsed man.[1]

As is the case to-day, one of the important posts in the medical schools of that early day was the chair of anatomy; a survey of the various medical schools of the time of Boerhaave and Haller indicates that any teaching of what is now understood by the term physiology was done by the professor of anatomy. Even Haller, who is now remembered as the immediate forerunner of modern physiologists and a great student of the functions of organs, made his living teaching anatomy, first in his native town, Berne, Switzerland, and later at the University of Göttingen, which George II of England, also Elector of Hanover, had just founded.

Haller's own published works furnish an excellent example of the close relationship which existed between anatomy and physiology and the dependence of the latter upon the study of structure as the *raison d'être* for its own study. Göttingen was provided with a very good anatomical theater, and from dissections made there Haller produced a set of most beautiful anatomical drawings which he published between 1743 and 1753. Vesalius had published his famous "Structure of the Human Body" a century before (1542), and even Haller's master Albinus had made drawings of interest to anatomists. These earlier works, however, were comparatively coarse, those of Vesalius made so because that master

[1] Quoted by M. Foster, in *The History of Physiology,* p. 285. Cambridge University Press, 1901.

had no microscope, and those of Albinus because only separate parts, such as a muscle, a nerve or a vein, were drawn. In Haller's plates were first shown the different nerves and vessels attached in their right position, and—what is most interesting—to each plate was added a complete account of the *function* or use of the part drawn. Haller's drawings were so accurate and full of detail and required so much time to prepare that in seventeen years, with all the help he had, he was not able to complete the description of the whole human body.

The word *physiology* comes from a Greek word which may also be translated as *natural philosophy*. Among all the many sciences which have developed from the scientific renaissance of the nineteenth century as offshoots from what was formerly called natural philosophy, physiology alone has continued to use the old mother term; in this particular instance, however, the word has been retained but with its meaning greatly altered. When one endeavors to determine the precise origin of the modern science of physiology by fixing upon a definite date or individual or discovery, he meets with a real difficulty: if his own scientific training has been essentially in or closely related to the field of chemistry, he may regard Lavoisier as the real founder of modern physiology; on the other hand, all the German students of the history of science have seen in the person of the great Johannes Müller the first of the really modern physiologists.

While such uncertainty appears to envelop the origin of this science, much interest does attach to the fact that in 1824—one hundred years ago—appears to have been given the first course of instruction in anything like what is now understood by the words *experimental physiology*.

In 1822, a young Bohemian scholar, Johannes Evangeliste Ritter von Purkinje, was called from a lectureship in anatomy at Prague to the chair of physiology at the University of Breslau. This young teacher, after following for two years what was to him the old-fashioned method of teaching this subject, decided to change the method. What greatly impressed Purkinje was the lack of any really exact knowledge concerning the functions of organs and the predominantly theoretical and speculative character of all work in this field. Few if any original experimental investigations were being made in physiology anywhere; the theories of Boerhaave, Haller and other old masters formed the subject-matter of discourses in this science. In 1824, therefore, Purkinje began illustrating his lectures by a variety of experimental demonstrations in order that he might arouse among his students an interest in physiology as a subject quite worthy of study for its own sake. So far

as can be ascertained, this was the first course in anything like experimental physiology.

As has so often been the case, this great heritage in science had its beginning under rather inauspicious circumstances. The available class rooms were not suited for such work as Purkinje wished to do. At first the demonstrations were held in a small room attached to the department of anatomy; later the physical laboratory was utilized for this purpose. In 1831 Purkinje asked the authorities to erect a special building for physiology, and this request was refused. Discouraged and despairing of finding a home for his growing subject in the laboratories of the university, Purkinje gathered up his instruments and departed with them to his home, where under most unfavorable conditions he contributed to the establishment of physiology as a separate science.

In 1830, after the university had refused to purchase a compound microscope for the physiology department, Purkinje, with the assistance of friends, secured one himself at a cost of fifty dollars: it was with this instrument that he first noticed the large cells of the cerebellum which bear his name.

Purkinje studied a variety of problems in the fields of physiology and anatomy. In 1823 he described the now well-known method of investigating the structure of the retina by observation of what is seen after a flame has been waved beside the eye; two years later, in a study of the origin of the bird's egg within the ovary, he made the important discovery of the germinal vesicle, which came to be known for a long time as the "Purkinje vesicle." His contributions to histology included descriptions of the structure of bone, cartilage, blood vessels, glands of the stomach and the ducts of the sweat glands, and culminated, from the standpoint of the interest aroused, in his announcement in 1837 at a meeting of naturalists in Prague of his discovery of the axis-cylinder of nerve cells. At this same meeting he expressed the opinion that the interior of many organs is composed of cells and nuclei, an opinion which may be regarded as a foreshadowing of the cell theory which Schwann's investigations definitely established only a few years later.

All these discoveries, together with Purkinje's growing fame as a teacher, led finally to the establishment by the government in 1839 of a separate institution for the teaching of physiology and for physiological research. For twenty years after his dream had thus been realized, this great teacher and investigator labored at Breslau. His service as an advocate had been successful; physiology had indeed come into her own.

Perhaps it would be an exaggeration to say that Purkinje's work at Breslau was the leaven which developed the loaf of physiology

in Europe; doubtless many other factors contributed to promote interest in the experimental side of this subject. The experimental method was being used extensively in other fields of endeavor, and each great discovery resulting therefrom in physics, chemistry or biology only served to give impetus to the forward movement which the scientific spirit was making. Mere mention of the fact that among Purkinje's contemporaries were such men as the chemists, Berzelius, Dumas, Liebig and Wöhler, the great mathematician and physicist Helmholtz, and the biologists Magendie, Schwann, Johannes Müller and others, should suffice to indicate how much the development of physiology after 1824 was due to a great wave of scientific endeavor which gained in power as each decade passed. With the growing interest in the search for new facts concerning the physical universe, physiology, together with the other sciences, profited, new institutions for the study of functions of organs developed and other illustrious names appeared in this field, among them Du Bois Reymond at Berlin and Claude Bernard at Paris. A detailed account of the advance in the field of physiology made after the establishment of the institute at Breslau would fill a large volume and would be essentially the story of modern physiology; certainly, after glancing over the accomplishments made during the past hundred years following Purkinje's course in experimental physiology, few will be found to deny that in this case at least "wisdom is justified of her children."

It is of interest to know something regarding the later history of the institute at Breslau. In 1859 Purkinje resigned his professorship and returned to Prague, where he took an active part in Slavic national movements and politics and gradually withdrew from endeavor as a scientist. His successor at Breslau was a young man, twenty-five years of age, Rudolph Heidenhain.

The fame which the Breslau Institute achieved under Purkinje's directorship was in no way dimmed in later years when Heidenhain was in charge. This great teacher and investigator, to use the words of Professor Carl Voit, "soon began and ever after displayed a fruitful scientific activity and trained many students to whom his conscientious and unflagging zeal for the work was an inspiring example; in such a manner did he fulfill the confidence placed in him to the highest degree." His studies of secretion were masterly, and included not only observations of the phenomena of secretion but histological examinations of gland cells both in a resting and in an active condition. The Heidenhain monograph concerning secretion, which forms a large part of the fifth volume of Hermann's Handbuch der Physiologie, is one of the classics of physiological literature. During these days of 1924 when over-specialization and

independent activity seem to be unduly emphasized and appear in some measure to affect the quality of research being done, Heidenhain's method of studying secretion may teach a lesson of the value of cooperation and completeness in physiological research.

Under Heidenhain's inspiration the great Russian school of physiology was founded. Pawlow's remarkable studies of digestion, for which he received the Nobel Prize, involved in part a method of operative procedure which was an improvement on a method devised by Heidenhain. Thus may the Pawlow laboratory at the Imperial Medical Institute at Petrograd and all its interesting contributions to physiological knowledge be regarded as the offspring of the Institute at Breslau.

Heidenhain's influence extended also to other parts of Europe and even to America. The prominent English physiologist, Professor Ernest Starling, now at University College, London, received his training for the study of problems concerning the lymph from the Breslau master. Among the American students who thus received inspiration for their life work may be mentioned Professors W. T. Porter of Harvard and Lafayette B. Mendel of Yale University.

How does the future outlook for present-day physiologists compare with that which Purkinje must have visualized? Of course one can only speculate as to what the Breslau teacher must have thought as to the possibilities in the scientific study of function. That new and important generalizations would be made, he must have realized, for we know that he was learning to think in terms of the cell theory several years before Schleiden and Schwann definitely established this conception. Purkinje's scientific career was full of achievement, and that fact alone may be taken as indicating that his was an active mind alive to the possibilities in this field and anxious to realize them.

Present-day physiologists frequently hear a note of pessimism regarding the future outlook of their science, usually from students who have not been taught the directions along which physiological advances are being made and stimulated to feel that they are caught in a great current of enthusiasm which will carry them on to new and worthy accomplishments. Surely the present outlook may be regarded every bit as promising as that which physiologists living a century ago faced. It might safely be said to be even more promising, for the circle of knowledge—to use Herbert Spencer's figure—has become much larger and the number of points at which the known touches the unknown outside the circle of circumference has correspondingly increased. A host of problems still remain to be solved; the vast majority of them have come into being simply

as a result of discoveries that have already been made. New interrelationships of organs remain to be learned; the mysteries of many ductless glands are still with us; the rôle of such an unsuspected factor in nutrition as sunlight furnishes numerous problems for the detective in physiology to unravel. These are only a few of the many possibilities of service for the advancement of physiological knowledge which are still open. Given the scientific insight which Purkinje had, and a similar undaunted spirit which insists on finding a way around obstacles, there is no reason whatever why the twentieth century physiologists can not hand on to their successors of the next century a heritage of achievement as large and as worthy as was received by them from Purkinje and his contemporaries, Heidenhain, Bernard and the many others of the past hundred years.

# THE ORIGIN, NATURE AND INFLUENCE OF RELATIVITY[1]

By Professor GEORGE D. BIRKHOFF
HARVARD UNIVERSITY

## II. The Nature of Space and Time

While the historical survey of the development of the concepts of space and time is of essential value, only a direct logical analysis on the basis of the known experimental facts gives a satisfactory understanding of their real nature. Such an analysis indicates clearly why the space-time of relativity can be considered more nearly adequate than the classical space and time, and yet equally concrete and simple. As a preliminary step in this direction, the elementary facts of everyday experience will be stated independently of any particular theory. These are the same, whatever ultimate theory may be required to explain the physical universe down to the last ascertainable detail.

A fundamental characteristic of everyday experience is its arrangement in the order of time. Sensations are experienced in succession, and are recalled in the same succession as that in which they were experienced. There is also a flow in external nature nearly parallel to the flow of the stream of consciousness in the individual. Indeed, for the events which come within the range of immediate personal experience, sensations and the corresponding events are correlated in time without further question. It is true that occasionally a slight readjustment of this naïve interpretation is required, as when thunder follows lightning and it is realized that sound takes time to travel. Owing to the extraordinarily large velocity of light, it is never necessary to make an allowance of the same sort for light in ordinary experience.

It is very important to grasp the truth that a complete and exact parallelism between sensations in the individual consciousness and events everywhere throughout the physical universe, implying absolute simultaneity, can not be established in any simple way. For example, the attempt to correlate events as happening when seen was abandoned when the successive eclipses of Jupiter's satellites were observed to take place at smaller intervals of time when Jupiter and the earth were approaching one another than when receding. This anomalous result was accounted for by Römer in

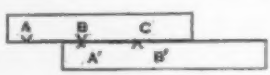
[1] Lowell lectures.

1675 by means of the finite velocity of light. The complete failure of such a correlation of events is brought out even more clearly by a hypothetical experiment like the following. Imagine that two observers $A$ and $B$ on two mountain tops, at a distance of one hundred miles from each other, send out flashes by means of revolving mirrors so that $A$ sees $B$'s flash simultaneously with his own. Then $B$ will see $A$'s flash about one thousandth of a second later than his own, and not simultaneously.

Thus it appears that the notion of absolute simultaneity of events requires physical definition by means of a technical process, and will have real meaning only if that process is unique. Our preconception that there exists such a significant correlation of events is based on the illusion of everyday experience that events happen when seen.

With these reservations, the notion of measurable time on the earth, call it earth-time, may be accepted as a working hypothesis. This time is measured with maximum precision by means of clocks at rest on the earth. Highly technical methods are used for synchronizing clocks far apart on the earth's surface.

In the stream of consciousness external objects called material bodies are identified and have a peculiar permanence. Among these are various nearly rigid bodies, possessing nearly constant size and shape. A piece of steel furnishes a good example. The comparison of such bodies by direct superposition leads to the concept of distance as determined by means of the ruler. In fact, take two simple rigid bodies of the very special type called straight edges. These can be exactly superposed in a variety of ways, and in particular so that any point of one coincides with a prescribed point of the other. Now let $A$ and $B$ be two points of the first straight edge, and let $A'$ and $B'$ be the two superposed points of the other. Place $A'$ on $B$, and let $C$ be the point of the first which falls at $B'$. Thus




we can mark the set of equidistant points $A, B, C, \ldots$ on the first straight edge, which can now be used as a ruler for measuring distances. Likewise, it is possible to construct a protractor for measuring angles. The laws of Euclidean geometry embody the results of approximate measurement in most convenient form. These laws are not to be taken as exactly applicable, particularly since no bodies are completely rigid.

An elaboration of these geometric ideas, in which the earth is taken as a rigid sphere of reference, gives rise to what may be called

earth-space. The position of a body in earth-space may be specified by giving three spatial numbers, *e.g.*, latitude, longitude and distance above sea level.

These systems of measurement of earth-space and earth-time may be usefully combined. Any terrestrial event may be completely specified by naming the instant of earth-time and the point of earth-space with which it is identified.

It is also evident from everyday experience that light is derived from definite material bodies like the sun, and occupies straight lines in earth-space in accordance with simple optical laws.

A physical theory will be held reasonable by the layman if it explains these properties of space, time and light, as well as any further physical facts that may have come to his knowledge.

Our concern is not with such a superficial explanation of the obvious facts of everyday experience. It is entirely with that fundamental kind of explanation which aims to provide a complete and exact account not only of these facts but also of those discovered by the more searching processes of the laboratory and observatory. How was it possible to advance in astronomy except by means of geometry which makes precise the spatial intuitions, and by use of the telescopic lens shaped with an infinite care so as to supplement the feeble powers of direct observation? By such means the physical universe has been found to be subject to highly exact law. As far as the underlying theory has been revealed to man, it appears ever more unified and grandiose.

Let us turn then to examine spatio-temporal law in some such spirit. It is well to begin here also by stating what are conceived to be the facts in the theory of absolute space and time hitherto accepted, and what are conceived to be the facts in the theory of relativity. In this way it will appear that there is a great deal in common to both, so that the divergences are minor by comparison and the new theory is equally simple and concrete. After so doing it will be possible to give intelligent consideration to the question of the logical analysis of space and time.

According to the classical theory there are two principal conditions which must be satisfied before the spatial comparison of material bodies (taken to be at the same temperature) becomes possible in any exact sense. All bodies in nature are elastic. Even a piece of steel lying upon a table is flattened by the force of gravity. Consequently, a first condition is that the bodies are small and at a great distance from any large bodies, so that such gravitational distortion is negligible. Furthermore, if such bodies are in a state of rotation, a similar condition of distortion will ensue because of the so-called centrifugal forces called into play. Hence it is necessary further

that there be no rotation. Under these favorable circumstances the body may be said to be undisturbed. When two undisturbed bodies are placed in contact they will continue in contact, and exact measurement by superposition becomes possible. The laws of Euclidean geometry will then be found to hold with maximum exactitude, according to the classical theory.

It is precisely the same in the theory of relativity. Small bodies at a considerable distance from any large one may not be rotating, so that no centrifugal forces are called into play, and then may be said to be in an undisturbed state. When placed in contact their measurement by superposition becomes possible, and the laws of Euclidean geometry will hold with the same degree of exactitude.

Even so, the degree of accuracy to be expected will not be complete if matter is atomic, because of atomic agitation. It may be assumed that the atoms themselves are made up of perfectly elastic bodies carrying electric charges, and the difficulty will thereby be turned. However, our knowledge of the nature of the atom is so incomplete that this is not much more than a subterfuge. It must be admitted that neither the classical nor relativistic theory gives any adequate account of the fine structure of matter.

Further measurement by means of light rays and Euclidean triangulation from any undisturbed reference body yields the position of other such bodies as if all these bodies were at rest in an augmented body or medium in which the rays of light occupied straight lines. Any point of this hypothetical body may be called a point of space, whether or not an actual material body exists there. If there is no such body the space is said to be empty at the point. There is sometimes a tendency to find a lack of reality in the idea of empty space. This is due to a vague appreciation of the fact that space like number is an abstraction, although both are extremely useful. The physical properties noted when a material body is brought to a point of empty space are entirely definite. If a lighted match is placed in empty space (*i.e.*, in a vacuum) its flame is quenched; if a small balloon full of air is placed therein it explodes.

It will be observed that the space which has been defined is the relative space about an undisturbed reference body. At this stage a difference of nomenclature between the classical and relativistic theories appears. Undisturbed bodies may be moving with respect to one another. In the relativistic theory the space defined by any one such body has precisely the same significance as any other, while in the classical theory a particular body is singled out as absolutely at rest, and is used to define an absolute space. Nevertheless, as far as the simple experimental methods used above are concerned, no difference will appear in the various spaces.

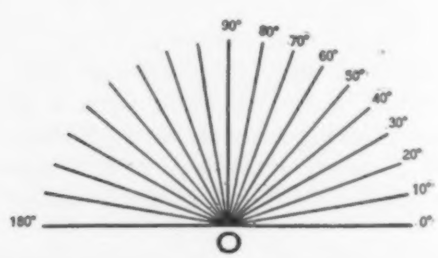
The laws of any such space have been stated to be Euclidean. It is distinctly worth while to pause and consider how simple and reasonable these laws are. The simplicity is often obscured by an effort at logical completeness, whereas if the logical demands imposed are only such as would have satisfied the early Greeks, geometrical laws are very easily understood.

Any (undisturbed) body is made up of parts. The smallest such part is indivisible and is called a point. The simplest body containing two distinct points is a straight line, an example of which is afforded by any ruler. Two such straight lines have the characteristic property that they will coincide throughout if they coincide in two distinct points. The plane is the simplest body containing three distinct points not in a straight line. Two planes will coincide throughout if they coincide in three points not in a straight line.

Plane geometry deals wholly with the relations of the parts in a plane. The facts concerning geometry in the plane (and indeed in space) can be taken to repose upon the following four assumptions.

I. Measurement of distance in a line can be made by means of the ruler.

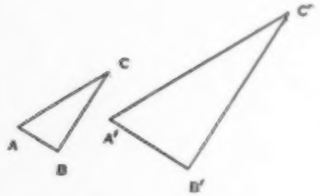

II. Measurement of the angle between lines can be made by means of the protractor.




III. One and only one straight line contains two given points.


IV. The plane is alike and even similar to itself in all its parts.

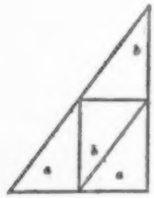
It is only as applied to triangles to the extent indicated in the adjoining figure that this assumption is required.




There are three propositions which follow readily from I–IV and are of central importance for the rest of geometry. We proceed to state and "prove" these briefly.

V. The sum of the two angles less than 90° in a right triangle is 90°.

The proof may be made as follows: Bisect the sides of the right triangle, by I. Join the three points of besection by three straight



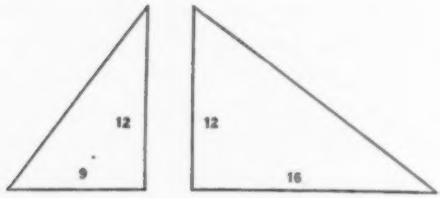

lines, as may be done by III. The three small triangles touching the three corners of the given right triangle are then clearly similar to that triangle in accordance with IV, with corresponding sides half as great. The same is seen to be true of the interior small triangle. Hence the angles labelled $a$ and $b$ are the same, and it is apparent that when these are added by means of the protractor in accordance with II, their sum is 90°.

VI. In a right triangle the side opposite the right angle is related always to the other two sides as in the case of the 3, 4, 5 triangle when

$$3^2 + 4^2 = 5^2,\ i.e.,\ 9 + 16 = 25.$$

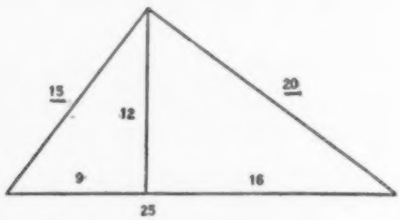
In the following discussion we shall show that a right triangle with sides adjacent to the right angle in the ratio 3 to 4, will have the side opposite that angle represented by 5. To prove it "in general" by the same method requires a little elementary algebra.

Consider the first triangle below in which the sides adjacent to the right angle are represented by 9 and 12 and therefore in the ratio 3 to 4. The numbers 9 and 12 are chosen instead of 3 and 4 to avoid fractions later.




This triangle, in which the remaining side is to be found, is clearly similar to the second right triangle with sides adjacent to the right angle in the ratio 12 to 16, as follows from IV.

If these two triangles be joined together so that the common sides coincide, a larger triangle is formed, of which the largest side

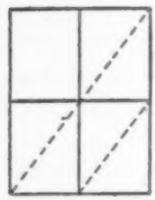



is 9 + 16 or 25 by I. The opposite angle is the sum of the two angles which appear in each of the two component triangles. Hence this opposite angle is 90° by V, and the triangle formed by the combination is a right triangle also. The new right triangle thus obtained is similar to the previous ones, since its angles are the same.

We are now sufficiently advanced in our knowledge to find the magnitude of the required third side of the first triangle. If that side is 15, the side 9 of the first triangle will be in the same ratio to this unknown side as that side, taken as belonging to the combined triangle, bears to the side 25 in that triangle, for 9/15 and 15/25 are both 3/5. On the other hand, if that unknown side is more or less than 15, these ratios can not be equal since one will be less than 3/5 and the other greater. Thus the third side is exactly 15. The remaining side of the large triangle is of course 20. Hence it appears that if the sides adjacent to a right angle in a right triangle are represented by 3 and 4, the side opposite that angle is represented by 5.

The last proposition which we shall state is the following:

VII. Any two lines at right angles together with all the lines at right angles to them form a network of rectangles.



The truth of this statement is apparent from the same figure as we used in the proof of V.

It will be found that with the aid of the propositions I–VII it is a very easy matter to establish the main facts of elementary geometry which are really useful in its simplest applications.

The way in which many salient truths of geometry are incorporated in ordinary experience is astonishing. A host of relations between points, lines, planes, etc., are constantly apprehended at a single glance. Consider the obvious fact that if a man walks three miles east and then four miles north, he will arrive at the same destination as if he had first walked north four miles and then east three miles. This is a truth closely related to VII. There are many similar obvious facts embodying geometrical truths.

The preceding formulation of the methods and laws of spatial measurement indicates that there is no difference in the meaning of these for the old or new theory. The same is true of temporal measurement in its most fundamental aspect, as will be explained immediately. It is only in the interrelation of space and time that a difference will be found to exist.

Let a tuning fork be in an undisturbed state in the space which it defines, and afterwards let it be set in a state of slight vibration. Each point of the tuning fork will then execute a periodic vibration about a point of that space, and thereby give a means of measurement of the lapse of time, *i.e.*, a clock. This is a perfect clock for any theory yet considered, as far as indicating lapse of time is concerned. A clock may be defined to be any small mechanism which serves to indicate equal intervals of time in its history. Clocks are omnipresent in nature, and the vibrating atom of any element is a kind of ideal clock. A clock merely measures duration where it is, *i.e.*, local time.

The simplest method of comparing time at different places on an undisturbed reference body is by moving clocks very slowly from one place to another, so as not to interfere with their action. An alternative equivalent method available for a distant event is to take its time of happening as midway between the time of a light flash from the body reaching the point when the event happens and the time of the reflected flash returning to the body. By these

means simultaneity and time, as well as space, may be defined physically relative to the undisturbed reference body chosen.

It is to be especially observed that the actual physical means employed in constructing the system of space and time reference are identical in both theories.

In nature no completely undisturbed bodies are to be found, although some are nearly in such a state. Some such body of reference is used to set up a specific system of space and time. The query naturally arises as to whether it is not possible to define space and time otherwise than by means of small undisturbed reference bodies. The answer is that the corrections to the above processes then required are extremely technical. Consider a heavy elastic sphere at rest in the absolute space of the classical theory; a first requisite for its use in space measurement is a complete knowledge of the theories of elasticity and gravitation. In all cases and no matter which theory is adopted, the path to highly exact spatio-temporal measurement otherwise than as stated is an extremely arduous one.

There is a slight difference in the spatio-temporal facts as these are conceived in the two theories. In the classical theory there is a particular undisturbed reference body, absolutely at rest, for which the statement of physical law assumes a superior simplicity. For it, light travels with the same velocity in all directions, and the shape and size of other undisturbed bodies in relative motion, taken at an instant of its own absolute time, are the same as if the bodies were at rest. In the theory of relativity every undisturbed reference body defines a relative space and time which has the same validity as that of any other such body. To counterbalance this increase in simplicity, however, there will exist a modification in the shape and size of an undisturbed reference body in relative motion as obtained by the indirect optical method, although there will be no modification if the dimensions are determined by direct spatial measurement.

It is particularly important to understand the modification to which the notions of time and simultaneity are subjected in the passage from the old to the new ideas.

In whatever way time may be determined, there is one property unconditionally demanded, namely, that it be impossible to modify events subsequent to their happening. If a wireless signal is sent from Boston at exactly 12 o'clock, it must not affect the New York receiving station before that hour. Similarly, if a signal is sent from New York at 12 o'clock, it must not reach Boston before that hour. However, since the signal only takes about one five-hundredth of a second to pass back and forth, it is clear that this

single demand upon the notion of time has fixed simultaneity to within very narrow limits. Now there is no known velocity which exceeds that of light and electricity. In fact, such a velocity seems contrary to the essential constitution of nature. Hence this kind of simultaneity appears to admit of no clearer definition. It may be termed ordinary simultaneity.

Absolute simultaneity is of an entirely different character. It implies the existence of a uniquely defined system of time measurement, with the aid of which the statement of general physical law obtains its maximum simplicity. For example, the law of gravitation formulated by Newton for classical physics will only hold if absolute time is used in the measurement of the gravitational acceleration of bodies. Such is the absolute simultaneity of classical physics.

On the other hand, if some specific system of time measurement is taken which yields the maximum simplicity of physical laws, and yet there exist other systems for which the same laws hold, then there is no such thing as absolute time or absolute simultaneity. This is the situation in relativity where the various systems of time measurement for undisturbed reference bodies are equally valid.

Thus the absolute time and simultaneity of the classical physics are replaced by a relative time and simultaneity dependent on the undisturbed reference body selected. The new simultaneity meets the fundamental demand first imposed.

There is a third kind of simultaneity which may be called transcendental. If the mind of Deity envisaged events in a temporal order, there would be defined a transcendental type. Similarly, if telepathic communication became possible and was found to be instantaneous in the sense that immediate communication back and forth was possible, then a species of transcendental simultaneity would be thereby defined. The validity of any transcendental simultaneity is entirely speculative, of course.

Einstein was the first to realize that it is not necessary to hold to the notion of absolute time. While absolute space had not been securely defined in the Newtonian theory, absolute time and its attendant concept of absolute simultaneity had never been questioned before. The cornerstone of Einstein's logical analysis of space and time declares the abandonment of the unnecessary hypothesis of absolute time, at least until it is called for by the physical facts. This implies that the notion of a rigid body must be held in abeyance, too, since the rigid body is conceived of as one which can move about freely, while the distance between a pair of its points at any instant of absolute time remains invariable. Such a statement becomes meaningless when the notion of absolute time is abandoned.

It may ultimately appear that one particular reference space possesses an entirely superior character for the statement of the laws of nature, and hence we may wish to call that space absolute. Likewise some one time may be preferable, in which case it may be called absolute time. Nevertheless, even if this happens to be the case, it will be well not to forget that such a particular choice is in no sense *a priori*, but is a matter of convenience and is arrived at through observed laws experimentally determined. It will be seen that this broader point of view insists that the universe of events has primary significance, and that it is desirable to characterize it in some fashion or other in terms of a three-dimensional space and one-dimensional time, together constituting what may be called the four-dimensional space-time of events.

It is idealized interstellar space which furnishes the *motif* for the new physics much as the idealized rigid body did for the old. In interstellar space are found vast stretches of empty space with here and there comparatively infinitesimal stars, moving with velocities relative to one another which may reach hundreds of miles a second. From these bodies light spreads out with inconceivable exactitude in every direction, and may be measured by means of that eye of the astronomer, the telescope, and his clock. Thus we are striving to interpret anew this world of the astronomer.

In the new model there seems to be only one type of measurable quantity, namely, that of duration at any one of the particles by which we propose to replace the stars and other bodies in interstellar space, in order to idealize and simplify it to the utmost. We propose to assume also that this duration is measurable by means of clocks at the particles, and has physical significance in the correlation of events happening at the particle. These clocks are taken to be identical in structure, so that if they are brought together they will run at the same rate. Such clocks exist in any physical theory which has been so far proposed.

Other particles may be at rest relatively to a given particle, as will be evidenced when a light signal always takes the same time to travel back and forth in the time measured on the clock of the first particle. Three particles relatively at rest will furnish a basis triangle, by which a space of reference may be set up through a light-signaling process only. The experimental rules adopted in doing this will be exactly the same as for three particles at rest in the ether of the classical theory, so that a particular time of reference will be obtained also.

It can now be *proved* that if space-time is alike in all its parts (which seems as natural and inevitable an assumption as that the plane of Euclidean geometry is alike in all its parts) then the reference space and time will have notably simple properties.

In the first place any such space will be subject to exact Euclidean laws of distance, and the angles obtained by lightsignaling methods. Here we obtain an optical interpretation of space akin to that demanded by Aristotle. Bodies at each instant of the chosen time fill a part of this space. It is found, further, that light will travel with one constant velocity in all directions, and other particles will travel with uniform velocity in a straight line, that velocity being less than that of light. It will even appear that the relative velocities of two particles is the same in the system of reference of either.

All the assumptions hitherto used are granted either in the classical or relativistic theory, and so it would seem that there is no denying the correctness of the logical analysis of the model presented by interstellar space, as far as we have proceeded with it.

The characteristic hypothesis of the theory of relativity now enters. Up to this point there has been a perfect parity between all the particles in interstellar space, and as far as that model is concerned it seems as inevitable to assume that this parity is complete as it does to assume that all points are on a parity in the Euclidean plane. The hypothesis of relativity merely asserts the parity to be complete.

It is desirable to be more specific. If a particle $B$ is moving relatively to the reference particle $A$, it is not hard to show that the apparent rate of $B$'s clock measured in $A$'s own reference space and time will vary in general with $B$'s apparent velocity. It is necessary to emphasize the fact that the rate of $B$'s clock so measured can not be in general the same as the rate of the clock at $A$ even in the classical theory, since in that theory it will only be the same if the reference particle $A$ happens to be absolutely at rest.

The characteristic assumption may now be stated in the more specific terms: The apparent rate of $B$'s clock in $A$'s space and time depends in exactly the same way on the relative velocity of $B$ whatever particle $A$ is.

It is even sufficient to demand that when direct visual observation of the rate of $B$'s clock is made at $A$, its rate is the same as that of $A$'s clock as observed visually at $B$, at least if $A$ and $B$ approach one another or recede in the same straight line.

When stated in this form the truth of relativity almost takes on a philosophic necessity. Imagine an interstellar space containing only two particles which are approaching each other. Why should not the relation between them be entirely symmetrical?

Now the space-time was highly idealized in that only point particles were assumed to be present. As a matter of fact such matter will always have spatial extension. If the spirit of the char-

acteristic hypothesis of relativity is to be maintained, it must be assumed that undisturbed elastic matter at rest in any reference space will always have the same size and shape, *i.e.*, that the back and forth signaling time between any pair of points of such a body will be the same under all circumstances. It follows that direct measurement by superposition of undisturbed elastic bodies may be employed. This substantiates more fully the claim made earlier concerning the rôle of geometry in relativity. Of course in actual experience the conditions are not met exactly, but the same difficulty is presented whatever theory is adopted.

A more careful examination of the consequences of the hypothesis of relativity shows that the respective times and spaces appertaining to any two reference bodies are the same if and only if they are relatively at rest. If they are moving slowly relatively to one another in comparison with the velocity of light, the differences between their respective spaces and times are extremely minute, and altogether beyond the range of observation under the conditions which usually obtain. Nevertheless, differences are found to exist, which justifies the caution which enjoined us from assuming an absolute time. It must be emphasized that the various spaces and times are effectively interrelated, so that time can no longer be thought of by itself.

It is unreasonable to expect any very obvious relation to exist between two different systems of space and time reference. Is it obvious how latitude and longitude on the earth would be modified if the magnetic pole were used instead of the north pole?

Thus the facts observed in everyday experience are as well accounted for by the theory of relativity as by the classical theory of space and time, while the former is superior in that it explains the futility of any experiment, like that of Michelson, designed to determine the absolute motion of any particle in interstellar space.

The theory outlined above is the special theory of relativity formulated by Einstein in 1905. The formulation took no account of gravitation, and yet it is possible to formulate a relativistic law of gravitation in harmony with that theory. Einstein's general theory of relativity of 1915 accounted for gravitation even more completely and profoundly.

It is often thought that there is something inconsistent about the special theory of Einstein. This is a mistaken point of view. In fact the theory of relativity, like Euclidean geometry, is a complete theory which may or may not be exactly applicable to the physical world in which we live. It is a self-consistent abstraction in any case, and the proof of its self-consistency is no more difficult

than that of elementary geometry. The self-consistency of geometry is usually assumed without question because geometrical relations are approximately realized in experience but that circumstance alone is not sufficient to make it rigorously self-consistent. Similarly, if we are willing to take the analysis of interstellar space (which certainly has physical existence) as proceeding intuitionally along the lines indicated above, we will not require a proof of self-consistency. If we refuse to accept this approach, an argument may be made of the purely arithmetic type available in geometry.

In our own world in which the relative velocities of bodies are very small when compared with that of light, nearly all the spaces and times are substantially identical. It may be conjectured that the laws of nature are such that, although relative velocities were high in the remote past, they have tended to diminish. Thus the low relative velocities now found may be accounted for. It is natural, notwithstanding, to look upon the situation just alluded to as an argument against relativity from the very standpoint of convenience so much insisted on hitherto, for it is certainly convenient to define some particular space and time with respect to which all the stars are in slow motion. The reply to this argument is merely that it is in no way indicated exactly what space and time are to be taken as absolute.

Thus the new point of view takes its start in the grandiose interstellar space of the astronomer, in which particles are in motion relative to one another, and light waves pass to and fro. The local clock and telescope are the only instruments of precision. If the parity of these particles be granted as a most reasonable hypothesis, the relativity of both space and time to the particular particle flows with the same inexorable necessity as prevails in the Euclidean geometry.

The situation is akin to that presented by the lines of symmetry in a closed oval. If the oval is circular, there are nfinitely many lines of symmetry passing through the center of the circle, and each is as valid as any other. The relativity of nature resides in a similar symmetry of natural law for various systems of spatio-temporal measurement. If a point on the circle is marked, there will be only one line of symmetry which will pass through the marked point. This is analogous to the fact that a specification of the reference body fixes the space and time completely.

# THE JOSEPH LEIDY CENTENARY[1]

## JOSEPH LEIDY'S INFLUENCE ON SCIENCE

By Dr. EDWARD S. MORSE
SALEM, MASSACHUSETTS

It was with some hesitation that I accepted the invitation of your committee to prepare an address on the subject of Joseph Leidy's influence on the science of his time. It is true that I am probably one of the oldest members of your academy, but it seemed to me that a member nearer home and consequently more intimate with Leidy's life and work would have been better chosen.

When Joseph Leidy began his studies there were only two centers of scientific study in this country, and these two centers were Philadelphia and Boston. In these two centers were established organizations that in a way paralleled each other. Philadelphia had its Academy of Natural Sciences and Boston had its Society of Natural History; Philadelphia had its American Philosophical Society and Boston had its American Academy of Arts and Sciences. The American Academy was organized in 1780, 37 years after the Philosophical Society, and both had among their members the immortal Philadelphian and Bostonian, Benjamin Franklin, who was the founder of the Philosophical Society.

In the transactions of these societies the memoirs and communications were of a similar nature. There were, it is true, isolated students of science in other parts of the country, but definite societies and museums there were few or none. In scientific work Philadelphia antedated Boston by nearly a century. The labors of those men, John and William Bartram, easily marked Philadelphia as the pioneer city in scientific work. Linnaeus corresponded with John Bartram and pronounced him the greatest modern botanist in the world. John Bartram is described by his son William as a man of modest and gentle manners, frank, quiet and of great good-nature. It would seem from this characterization that the mantle of John Bartram had descended on Joseph Leidy. It was in the territory about Philadelphia that the Bartrams had roamed and studied, and it was over this same region that Leidy found the material for his work in its woods, fields and ditches.

Leidy was a naturalist in the broadest sense and his scientific contributions to the number of hundreds embraced the wide field of comparative anatomy, zoology, botany, paleontology and mineralogy. In

[1] Papers read at the Academy of Natural Science of Philadelphia on December 6, 1923, to celebrate the centenary of the birth of Joseph Leidy.

the last century throughout the entire range of the animal and plant kingdom the country was rich in undescribed species. To the young naturalist the fascination of discovering and describing new species was overwhelming and some there are who have never outgrown this lust. What wonder that the zoologists and botanists of that era devoted their energies to the work of detecting and defining new forms. Agassiz used to say that the species described typified the mason who supplied the bricks for the edifice, an important work to be accomplished but followed by the same disaster if done improperly. With the growing scarcity of undescribed species the same kind of mental energy is now-a-days concentrated on defining new genera, and this tendency is becoming so accentuated that one may predict with certainty that ultimately every species will have its own generic name, and in print accompanied by an appalling synonymy. As the curious rules of nomenclature permit of more than one specific name we shall soon have—but enough of this.

Joseph Leidy, in the midst of this greed and rush for new species, steadily pursued his researches on the habits and anatomical details of creatures embracing the entire animal kingdom from the lowest rhizopod to the highest mammal. He described the new species and genera as they naturally revealed themselves in the course of his investigations, but this work was subsidiary to his greater studies. One stands amazed at the wide range of his observations. The diversity of his work began when he was comparatively a young man. Others will address you on this subject, but I can not refrain from calling your attention to the indices of the Proceedings of this Academy for the years of 1846, 1847 and 1848, to illustrate the breadth of his studies; he it was who determined that America was the ancestral home of the horse. His profound knowledge of the osteology of mammals enabled him to identify a fragment of a fossil tooth as belonging to a species of rhinoceros. Naturalists and paleontologists were skeptical as to the rhinoceros ever having lived in America, yet, later, in the same region in Nebraska, by a remarkable coincidence, the fossil skull of an unmistakable rhinoceros was exhumed and the remains of the very tooth was found embedded in its jaw.

That eminent zoologist, William K. Brooks, in a memoir on Leidy says, "He laid with the hands of a master the foundation for the paleontology of the reptiles and mammals of North America, and we know what a wonderful and instructive and world-renowned structure his successors have reared upon his foundation." Leidy's first communication to the Academy on Paleontology was in the year of 1848, and his last communication on the subject was in the year of 1888. Nearly a third of his memoirs to the number of 216 were devoted to this subject alone, absorbing nearly forty years of

his life. This task would have given him the world-wide reputation he sustained as a great naturalist, but at intervals during this prodigious work he found time to make investigations in many other and widely varied subjects in the animal and plant kingdoms, accompanied by innumerable drawings of the utmost delicacy and refinement. At a recent celebration, in Philadelphia, of the centenary of Louis Pasteur, Dr. Robert A. Hare, in a felicitous speech at the dinner given on that occasion, pointed out a number of striking parallelisms in the lives of Leidy and Pasteur. It was, indeed, a high honor to sustain resemblances to this immortal genius, and one who knows the history of these great men realizes the justice and accuracy of these comparisons.

Leidy's memoir entitled "Flora and fauna within living animals," accepted in 1851 by the Smithsonian Institution and published in its transactions, contains a long introductory chapter in which is discussed questions of the origin of life, spontaneous generation, years in advance of Pasteur and Darwin and anticipating many of their conclusions.

In an address given by Dr. Henry C. Chapman, at the unveiling of the statue of Leidy, in Philadelphia, in 1907, he quotes from a memoir by Leidy, in 1853, written six years before the appearance of Darwin's great work on the origin of species, and says. "Where, it may be asked, can there be found in the whole range of biological literature a more concise record of the origin of life, the extinction of species, the survival of the fittest, in a word, of Darwinism?"

I remember attending a stated meeting of this academy over fifty years ago. Leidy and other naturalists were present and the charming atmosphere and genial discussions reminded me of similar meetings of the Boston Society of Natural History at the same period: an identical group of kindly men, the amiable discussions without a tinge of acrimony remains a lasting impression. The days of Gilbert White have long since vanished. Have we also lost the stage typified by Leidy, Conrad, Lea, Haldeman, Meehan, Morton, Allen and others of like character? To a young collector of natural history objects it must have been an inspiration to attend these meetings and to realize that scholarly men were soberly discussing the habits of a common worm, or the structure of a beetle's leg. To see spectacled gentlemen seriously admiring little shells in a pasteboard tray, to appreciate how a well-written label added dignity to a trifle; to further realize that the little collection of natural history objects which they had brought together and for the collecting of which they had been laughed at or sneered at by unsympathetic neighbors were regarded by those men with interest and respect. Alas! these blissful days have passed. I know by my own

experience that the collectors' interests have vastly changed since those days. In the middle of the last century private collections of shells and other objects of natural history were not uncommon. The spirit of collection still survives, but other classes of objects claim its attention—postage stamps, coins, book-plates, etc., are deemed worthy of accumulation. The same tendencies are recognized in England. An eminent English authority in a report to the Liverpool Free Museum laments that "private collections are failing in Liverpool and all around, and teaching is hard and hardening in its results."

Leidy's profound range of knowledge, coupled with his willingness to answer inquiries, made him eagerly sought for, and crystals, precious stones, flowers, fossil forms and all kinds of animals were submitted to him for identification. It was the personal contact of a great master with students of every branch of natural history in which Leidy's influence on the science of his time exerted its greatest effect; questions answered, difficult attributions explained, obscure points made clear, all with a generosity of time, a kindliness of heart that left grateful and lasting impressions on the student. As a young man I marveled at the delicacy of his drawings, particularly his drawings made under the microscope, especially those depicting the anatomy of the terrestrial mollusks of the United States. He used a peculiar enameled paper, of which he gave me a number of sheets, and a metallic pencil. His gentle and cordial manner won my heart at once, and I went back home greatly encouraged in my studies by this simple interview. His broad and unprejudiced attitude was well shown in an incident which occurred at the meeting of the American Association for the Advancement of Science, at Troy, New York, in 1871. At this meeting I gave my views as to the systematic position of the brachiopods, endeavoring to show that these animals had no relation to the branch of mollusca with which they had always been associated, but that their affinities were with the class of worms. Leidy was at this meeting, but not a word in protest was expressed by him in contradicting these views; not that he agreed with me, for he did not, but, as I believe, from mere kindness of heart he refrained from combatting my heresies publicly. After the meeting, however, he came to me and quietly protested my views. I remember very clearly he urged me to save what little reputation I might have acquired as a student of mollusca by warning me to be sure of my ground before publishing. I confessed to him that perhaps I had not made myself clear in my extemporary remarks on the subject, but when he had seen my memoir with its illustrations and arguments it might possibly modify his views. Some months after in acknowledgment of my

pamphlet I got the following note from Leidy. "I have just finished reading the little book you so kindly sent me on the 'Systematic Position of the Brachiopods.' These, I think, you have clearly proved not to be true mollusks and to be more nearly related to the worms. It is singular how long mere outward resemblances deceive us and how reluctant we become to be undeceived."

Dr. Amos Binney, of Boston, published a work in three volumes on the "Terrestrial Air-breathing Mollusks of the United States." All the species then known were illustrated by steel engravings done by a Philadelphia artist. It was for this work that Dr. Leidy made a study of the anatomical structure of many species and the drawings illustrating these details for delicacy and beauty have never been equaled. Ten years before this work appeared Dr. S. Stehman Haldeman, a member of this academy, published a monograph of the "Air-breathing Fresh Water Mollusks of the United States," illustrated with colored steel engravings, so perfect that for accuracy and beauty they have never been surpassed. Of the 86 species figured half of them had been discovered and named by Thomas Say, a Philadelphian. Dr. Binney regarded Thomas Say as the earliest scientific naturalist that this country had produced. Dr. Binney was a Bostonian with all the proverbial pride of that city. He dedicated his great work in the following words:

> To the Academy of Natural Sciences of Philadelphia, to whose founders is due the first effective impulse given to the study of natural science of North America, and whose labors have been mainly instrumental in developing the natural history of this country.

It is interesting to note that Dr. Binney's son, William G. Binney, inherited the tastes of his father and continued his father's investigations with the greatest success; it is further interesting to observe that young Binney gravitated to Germantown, near Philadelphia, where he pursued his studies to the end of his life. Here he found as his associates, Lea, Conrad, Haldeman, Tryon and other distinguished students of mollusca, all members of this academy. Superadded to all these advantages he had access to the academy's collection of shells—one of the greatest in the world, and now in charge of Dr. Henry A. Pilsbry, one of our most distinguished malacologists.

In a preparation of this brief address it has been difficult to refrain from trenching on the grounds of others who honor us with their presence; in truth, the ground is broad enough for all, but it was necessary to understand the transcendent merits of this humane and world-famed naturalist in order to realize the profound influence he must have exerted on his colleagues in this center of scien-

tific culture. A standard was set by Leidy in every department of natural science and however feebly this standard may have been attained by some, an insensible pressure must have been continually exerted by the work of this great man in their midst. Philadelphia undoubtedly owes to-day its supremacy in natural science and the exalted character of its scientific institution to the work and example of this distinguished scholar.

## LEIDY'S ZOOLOGICAL WORK

By Professor H. S. JENNINGS

THE JOHNS HOPKINS UNIVERSITY

So many-sided was Leidy's work, even in the single field of zoology, that whoever examines it must be skeptical of the adequacy of his own impressions. And particularly will he be skeptical as to the adequacy of any brief unified presentation, such as it falls to my lot to attempt. The most I can hope is to illuminate a few of the many facets which his work presents.

Leidy seems to have attempted and carried out to a remarkable degree of success the bold project of forming for himself and communicating partly to others, a detailed picture of the entire living world in its natural relations to the environment. His zoological work is part of that effort. We find from him contributions on almost every one of the main groups of animals; and touching most aspects of their biology. The subjects of his published communications range from man and the other vertebrates down through the insects, arachnids, crustacea, annelids, rotifera, flatworms, nematodes, tapeworms, bryozoa, mollusca, echinoderms, coelenterates and sponges, to the lowest of the protozoa; and they deal with the structure, habits, reproduction, distribution and general biological relations of these creatures.

In his youth, we are told, Leidy's aptitudes were such that his father designed him for the career of an artist (though this design was overruled by the family court of last resort). In this fact lies, I believe, the key to an understanding of many features of his work in science. His work is largely a *portrayal* of nature as seen by a thorough scientific artist. It is artistic in spirit, not merely superficially, in the beautiful figures with which he illustrated it, but in its essential nature. His manifold contributions, of which between 500 and 600 are listed, may be compared to the studies from nature by a painter, ranging from the quick sketches in a few strokes, represented by his hundreds of brief communications to the *Proceedings* of the Academy of Natural Sciences of Phila-

delphia, through pictures of all grades of working out, up to such great and finished masterpieces as the volume on the "Fresh Water Rhizopods." All are mainly pictures of nature, not analytical treatises. To him biology was not, as it now is to so many, so much a series of problems to be solved as it was a continent to be explored, a landscape to be portrayed. There is little of experimental or statistical or conceptual analysis in his work; it contains few hypotheses or generalizations. Where such are mentioned, they are not as a rule looked upon as part of the material for investigation; not as matters to be tested by analysis of his data; he rarely draws from his observations conclusions concerning them. His work is a portrayal of nature as he saw it, and no one else in America has made for himself so uniformly worked out a picture of the world of organisms.

Now, of course, this means a scientific picture, with all that that implies of minute study of details, and of portrayal of these details in correct relations; his work was as far as possible from impressionistic. And the comparison with the work of the artist must of course not be pressed too far. Leidy had, too, the instincts and the capabilities of the analyst, the experimenter, the generalizer. In some of his earliest work, as we shall see, the products of those instincts are conspicuous; but in his later zoological work they are largely absent; and for a brief characterization, that as the scientific artist of nature is, on the whole, one that helps.

For his completer pictures in zoology he chose those groups of organisms which came naturally to hand. Living in Philadelphia, these were chiefly the inhabitants of the fresh-water ponds and streams. Contributions there are, and important ones, on the invertebrates of the land, of the air and of the seashore; on parasitic organisms and on marine animals; but of life in the fresh waters his pictures are most numerous and detailed.

Outside of that field, however, lie his first really worked out pictures, the paper on the parasites of the termites, and that on "A Flora and Fauna within Living Animals," published in 1853. Both are captivating examples of zoological landscape painting, illustrated with the beautiful figures so characteristic of Leidy. But it is in the latter paper that the young author shows his natural bent toward experimentation and toward generalization. In an introductory section on the "laws of life in general," he mentions experiments which he had carried out on spontaneous generation; on endosmosis, and on the effects of ingesting infusoria and bacteria. In the latter he tried on himself the somewhat hazardous experiment of swallowing water containing, as he says "*Monas, Vibrio, Euglena, Volvox, Leucophrys, Paramecium, Vorticella,*

etc.," but with no detectable consequences. It was in this paper, too, that he gave, six years before the publication of Darwin's *Origin of Species*, his famous statement of the doctrine of evolution, of his belief in the origin of the organic from the inorganic and of the general course which he conceived the development of the animal world to have followed. This striking statement should perhaps be quoted in any general characterization of Leidy's zoological work. He says:

> An attentive study of geology proves that there was a time when no living bodies existed on the earth. . . . Living beings, characterized by a peculiar structure and series of phenomena, appeared upon earth at a definite though very remote period. Composed of the same ultimate elements which constitute the earth, they originated in the pre-existing materials of their structure. . . . The study of the earth's crust teaches us tha very many species of plants and animals became extinct at successive periods, while other races originated to occupy their places. This probably was the result in many cases of a change in exterior conditions incompatible with the life of a certain species, and favorable to the primitive production of others. . . . Probably every species has a definite course to run in consequence of a general law; an origin, an increase, a point of culmination, a decline and an extinction.

Such statements, made in 1853, bring again to realization what we sometimes forget, that what Darwin did was not to propound a new idea, but to give overwhelming evidence in favor of a theory that was familiar to all intelligent students and that was held by many of them. In his later works Leidy largely restrained himself from any tendency to generalize; one wonders whether from conscious principle, as from something cheap and easy. Experimentation also becomes infrequent, though it does occur. One suspects that if Leidy had lived in the period when biology became more analytical, he might have become another Driesch or Morgan.

Other detailed portrayals of nature are found in his work on the anatomy of the terrestrial gastropods, in Binney's *Terrestrial Mollusca*; and in his papers on *Urnatella*, on *Belostoma*, on the walking-stick, on *Corydalus*, on marine sponges.

But for zoologists his great masterpiece is the volume "The Fresh-Water Rhizopods of North America"; some consideration of this will bring out the characteristics of his zoological work. In general, scientific books and papers are among the most evanescent products of human activity. The advance of knowledge soon renders them out of date; they continue to live only in that they have supplied nutrition to their successors. But such a work as Leidy's "Rhizopods" brings to realization the fact that perhaps the most permanent form of scientific literature lies in a full and accurate portrayal of some part of nature, without analyses of problematical matters, without hypotheses or generalizations. Generalizations

soon become inadequate; experimental and statistical analyses are superseded or become useless when once the conclusions on which they bear have been established or disproved. But an adequate account of a group of organisms in its relations to the rest of nature is like an adequate description of anatomy, or like the working-out of some of the constants of nature, like the computation of a table of logarithms—of each of these we can say that when 'tis done 'tis *done;* it need not be done again. This seems the case with Leidy's "Rhizopods"; it is a section of nature permanently preserved to us. From the first it was, and it will remain, a delightful guide to acquaintance with these strange and beautiful creatures, which sum up in miniature the riddles of life. The student with a microscope, a pond and Leidy's "Rhizopods" need envy the pleasures of no man in the world. And thousands of students have been in this happy situation; no other influence has been so potent in promoting acquaintance with the natural history of these lowest of animals. Even without the pond and the microscope, the volume is, for its illustrations, a delight to the artistic eye. But much more than all this; although zoology has become more analytical since Leidy, such a portrayal of nature is not left behind; it becomes only the more valuable. It presents to us with direct vividness the problems which nature sets, and it becomes a quarry of materials for work on those problems. The fundamental questions of biology—the problems of metabolism, of movement and behavior, of development, of reproduction, of heredity—come sharply to a point on the activities of the protoplasmic substance; here many students are directing their efforts. But conclusions in general biology and general physiology are often vitiated by the narrowness of the base on which they rest. A phenomenon is studied minutely in some one organism, and the conclusions drawable are held to be general laws of nature; whereas they are often but special peculiarities of that particular creature. Nowhere is this common error more easily fallen into than in connection with the lowest organisms, as I know to my cost.

Protoplasmic movement, for example, is indeed shown in *Amoeba* of the *proteus* type; and here zoologists usually study it and draw conclusions concerning it. But Leidy will show it to you occurring in many other naked protoplasmic masses differing greatly from Amoeba proteus; in Amoebae with tough skins, almost unchanging forms and rolling motion; in *Dinamoeba,* with its seemingly permanent outer layer studded with projecting points; in the gossamer-like branching and net-forming *Biomyxa vagans,* which compared to Amoeba proteus seems almost as simple as does *Amoeba proteus* compared to an insect. I still recall the feeling of

awe which this organism gave me the only time my microscope came across it, while following Leidy as a guide. *Amoeba proteus* when adequately studied doubtless does contain the secret of protoplasmic motion, just as the flower in the crannied wall contains all the secrets of God and man. But its outward manifestations of that motion may be gross and specialized peculiarities; I personally believe that they are. The student of that subject could not do better than take Leidy as his guide; study minutely protoplasmic motion in the extremely diverse types which he presents; in the various species fitted for the study of special points. Only so is there a chance to distinguish special peculiarities from general laws of protoplasmic movement.

This is typical of the situation with respect to other biological questions; with respect to nutritional problems, to the conditions of existence, to developmental and genetic problems. The student who tries to keep alive and to cultivate the various creatures of Leidy's pages finds the different ones so narrowly dependent upon particular nutritional conditions, and on other conditions that he becomes skeptical of general nutritional laws based upon the study of one or two kinds of animals. In relation to genetic processes and problems, Leidy's work is particularly alive and suggestive. He presents us in unrivaled figures and descriptions a vast assortment of different forms, exemplifying every degree of diversity. He classifies these, he distinguishes individual differences, varieties, species, genera, families, orders. What do these things mean genetically? Leidy tells us that in these creatures he believes that "no absolute distinctions of species and genera exist": that he finds the species "by intermediate forms or varieties merging into one another." In his accounts of particular species, he often emphasizes that they do thus merge by intermediate forms into others: also that within the single species or variety there are great variations of structure. What is the observational or experimental content, potential or actual, of these propositions? Does the "merging into one another" of two species mean that the individuals of one may and do produce at reproduction individuals of the other? If not, what does it mean? Are the individual diversities, are the so-called different varieties, to be found among the descendants of a single individual? Or are such diversities hereditary; permanent throughout the generations? Or may there be a process of gradual change, so that only after many generations may one variety be obtained from the other; or only after many generations may one break into several? What part does the environment play in all this?

These questions press upon us in studying Leidy, as they do when we study nature, but, like nature, he gives us no answers to

them. All may be attacked by simple and direct methods, and positive answers may be obtained; for most of the questions long periods of time are not required. The animals must be dealt with individually, cultivated individually, their pedigrees kept from generation to generation, as we do with rats; they must be handled as we handle rabbits. This can be done—it has been done for a few of Leidy's organisms—but only sufficiently to open up a vista of vast extent for future work. Some of the individual diversities within a species, it is found, have no hereditary basis—a parent of one type produces offspring of another—the environment may play a large part in determining which shall occur. But this is held between definite limits; and other individual diversities within a species *are* permanent and handed on from generation to generation. Each species contains numerous slightly but permanently differing strains. The so-called varieties do not produce one the other, and individuals of one so-called species do not produce individuals of the other. The merging into one another of which Leidy speaks is therefore not an experimental, a genetic, concept. Yet by long continued breeding for many generations, single strains are seen to gradually differentiate into slightly diverse ones, strains whose diversities are thereafter inherited from generation to generation. How far may this go? We do not know; as yet one "species" of the systematist has not been produced from another; nor even perhaps one "good" variety. I fear that we are still in the situation which Leidy summarized in 1853 when he said that "No one has ever been able to demonstrate the transmutation of one species into another"—even for species which he described as merging into one another.

Such work and other work must be carried out and greatly extended for all the different types which he describes. Their different methods of reproduction must be determined; we know almost nothing of their sexual processes or in most cases whether such exist. The nature and results of their seemingly inchoate, imperfect form of conjugation must be discovered; the details of their cytological processes worked out. Until all these are carried out to some level of attainment, the significance of the observations along the other lines must remain uncertain. For all this research, Leidy's work is a mine of suggestions and an indispensable guide. Many other of Leidy's contributions are similarly basic and suggestive.

But these examples must serve for many. In the brief time available I have attempted only to emphasize and illustrate the vitality of such work as Leidy's. In spite of the fact that it does little in the way of analysis and generalization, that it answers few general questions—perhaps indeed in virtue of that fact—it partakes somewhat of the inexhaustibleness of nature.

# LEIDY'S PALEONTOLOGICAL AND GEOLOGICAL WORK

By Professor WILLIAM B. SCOTT
PRINCETON UNIVERSITY

BEFORE I begin what I have to say on the subject of Leidy's work as a paleontologist, I think you would be interested to hear a letter, or at least a part of it, which I received some weeks ago from Sir Archibald Geikie, secretary of the Geological Society of London, and for many years director of the Geological Survey of Great Britain. In asking me to represent the Geological Society of London, of which I have the honor to be a member, he wrote this note.

> On the part of the Geological Society I am desired to inform you that we have been invited to send a delegate to the meetings that are to be held in connection with the commemoration of the centenary of Joseph Leidy, by the Academy of Natural Sciences of Philadelphia in December next. As foreign secretary I am desired to write you and express the hope that you will honour us by being our representative at the Joseph Leidy Celebration.
>
> The writing of this letter reminds me of my close intimacy with Joseph Leidy and the many friendly letters that passed between us. Should there be any representative of his family at the celebration in December, would you tell them how cordially I join in doing honor to the memory of one whom I reverenced and loved?
>
> With best wishes for your health,
>
> Very truly yours,
>
> ARCHIBALD GEIKIE

I may be pardoned, I hope, for continuing a moment or two on the personal note which this letter strikes. Osborn and I, in our senior year at Princeton—I ought to call him Professor Osborn, I suppose, but it is difficult to speak respectfully of one's classmates—you see, we know them too well—spent a large part of that year getting ready for the first of the expeditions to the Far West, as our hope was to get into the Fort Bridger country of Southern Wyoming. That immediately brought us into a study of Leidy's great monograph, published a little while before by the Hayden Survey, into attempting to learn what we were going to find in that country, and that went further to personal communication with Dr. Leidy. And after we had been in the Fort Bridger country in the summer of 1877 and made a large collection there, we spent a large part of our graduate year—1877 to 1878—in working up that collection, and we were constantly running to Philadelphia, and to the Academy, to see Leidy's types, to compare our material with that which he had described and named, and to ask his advice and his help. And though we were mere tyros, beginners, utterly in-

significant, he was invariably as kind and considerate and thoughtful and as lavish in the gift of his time as though he had had nothing else to do. I look back to those early years, when I began my professional career after having been in England and in Germany and had come back to resume my work at Princeton—I was constantly coming to see him, constantly referring problems to him for his consideration. Always, invariably, I got the help I wanted, and it was in such a contrast with the attitude of mind of many other distinguished men of science of that day. They were very "standoffish," many of them—they held you at arm's length and would tell you just as little as they could. I remember asking one very prominent paleontologist whether there were good collecting grounds at a certain region in Kansas. "Well," he said, "there were before I got there." That was all the satisfaction I could get out of him. Leidy wasn't a bit like that. What he had was yours if you asked him for it. And he had that sweetness and gentleness of personality that is so attractive when united with greatness. I have known a few great men in my life and, without exception, they have been men of extraordinary simplicity, without any airs, or graces—without any "side," as our English cousins put it. Huxley was another like that, only, living in London, he had to protect himself more than Leidy did here, because if he hadn't put a fence between himself and the public, he would have had no time for his work.

That is the note I want to leave with you, namely, that of extreme simplicity of character, of kindliness, of helpfulness, of feeling that his time belonged to any one who asked for it, if the object of that asking were not mere frivolity, but honest work.

Now it is a curious thing that both in Great Britain and in the United States the medical profession was until well into the nineteenth century—in fact the latter third of it—the only doorway to the study of zoology and paleontology. In England, the two greatest names in vertebrate paleontology that immediately stand out to any one who knows anything of the subject are Owen and Huxley. Both of them studied medicine. Huxley began his career as a ship surgeon and made a long voyage in a naval vessel which was as significant for his future as was the voyage of Darwin. The observations that he made on that voyage made a zoologist of him. Also, he did high class paleontological work.

Owen did perhaps more than any other man who ever wrote English in the volume and variety of the work which he published in vertebrate paleontology. He, too, began as a student of medicine, and his father-in-law, Dr. Cliffe, induced him to take the curatorship of the Museum of the Royal College of Surgeons in

London, from which he gained that amazing knowledge of comparative anatomy which made him a natural student of paleontology. The treasures which began to pour into England during the middle of the nineteenth century, from various exploring expeditions, were collected not only in Great Britain but throughout the world, and sent to Owen for description. The same thing was true in this country. All our early paleontologists were medical men, and it is therefore no mere coincidence that the center of paleontological work in this country, other than that of shells—invertebrate animals—was in Philadelphia. The first of them was Dr. Wistar—Caspar Wistar, whose name is held in such well-deserved reverence in this city yet, and the Wistar Institute is named for him.

Then there was Dr. Harlan; then came Leidy. Over in New York the same thing was true. The two naturalists who were most prominent in New York were Dr. Samuel L. Mitchill, a most extraordinary all-round person, and, I may say, because I am connected with his family, perhaps the most conceited man in the North American continent. He translated the "Theory of the Earth," and he said he did it at the request of President Jefferson who, he says, "remarked to me on one occasion that I was the only man in the country able to do it"—and he didn't add anything to that. He thought it was enough. "But," he thought, "I agree with Jefferson."

Samuel Mitchill was senator from New York and also was a very distinguished physician and also one of the earliest of New York naturalists, in the description not only of fossils but also of the living fauna of the state.

Dr. James deKay—I hope he didn't pronounce his name the way it is spelt—I wrote just the other day—(I see I can't get this off as my own, because the gentleman who gave me the facts is present in the audience; he is Dr. John M. Clark, the very distinguished superintendent of the Geological Survey of New York and the State Museum)—I wrote him the other day and asked him a simple question and he just showered information on me—namely, that deKay, who had charge of the botany and the zoology of the great New York Survey, was also a physician and deKay was one of the first men, I think, who ever described an American fossil horse. Thus, naturally enough, Leidy fell into the tradition because his tastes were that way, his interests were that way; as early as 1833 Sir Charles Lyell, the founder of modern geology, was in this city and he visited Leidy and told him: "Stick to paleontology. Don't bother with medicine. Stick to paleontology. That is your future." Well, Leidy didn't take his advice. In fact, under the conditions of those days it wouldn't have been feasible for him to

do so, because, like most of us, he had his living to make, and it could not have been made in those days by paleontological work.

Throughout his life, Leidy primarily was interested in human anatomy and he remained, almost to his death, professor of human anatomy in the Medical School of the University of Pennsylvania, but he felt always that man was only one of the vast multitude of animated beings. He told me once, laughing in that gentle way of his, of a student who came to him after a lecture and said, "Now, you don't really mean, professor, that man is an animal?" and Leidy answered, "Do you think he is a vegetable?" He, therefore, extended his work, as we have been told this morning, in all possible directions. Everything that lived had a fascination for him, and he wanted to learn not only its exterior appearance, and its habits, but he wished to know its structure, and in this way, quite unintentionally, he fitted himself to become the pioneer of vertebrate paleontology, which he was.

Now I could, of course, go on for a week, if necessary, if you just had the courage and patience to stay and listen, telling you a lot of technical details, but I have not the slightest idea of doing anything of the sort. It is Leidy's early history that I want to point out to you, and I think this audience, which is not composed of paleontologists entirely, is more interested in the influence which Leidy had in the development of this subject in which America has become supreme, especially in the vertebrates, throughout the world. All such things have their explanation and the reason of American supremacy in vertebrate paleontology is because of the vast material which this continent possesses. My friend Osborn has been lately getting over into Asia and finding equally great treasures there, and the expeditions which have just come back from China have brought us incredible and delightful treasures. Some of you heard of them in the newspapers. They have found dinosaur eggs. I hope the museum won't make any attempt to incubate those eggs, for it would be a misfortune to have dinosaurs on earth again. We are well rid of them.

Leidy's first publications in vertebrate paleontology dealt with certain bones found in a cave near Natchez, Mississippi. It was thought at first that these were human bones, because in those days and for a long time afterward every bone was human. There was a man who went through the southwest with a mastodon skeleton that he had mounted like a man, on two legs. The top of the skull was gone, so he restored the skull with a piece of rawhide like a human skull and exhibited it from town to town as the skeleton of a giant. And he had a trunk full of certificates from doctors in good practice that those were human bones.

These bones that were found in the cave were sent to Leidy, and they turned out to be bones of a curious ground-sloth, and it is interesting to note that it was on the ground-sloth that American paleontology began. The animal was described and named by Thomas Jefferson in 1805, or, that is, the volume of the Philosophical Society's Transactions appeared in that year—the paper, I think, was written about 1797, and in it he stated that the clawbone of a gigantic sloth was that of a gigantic lion. It wasn't much of a blunder. The resemblance is quite close, and Jefferson, of course, was an amateur. He didn't pretend to be anything else, but it is a paper that is full of interest from many points of view. I am sorry that my time does not permit me to tell you something about it.

Then the giant ox tribe attracted Leidy's attention, and he published a monograph on that. He published a monograph on the peccaries that were found in various parts of the country. Now, they are never found north of Texas.

Those surface things naturally came to him, but the fossils upon which his greatest reputation rests, I think, are the materials found in the Tertiary deposits of the Far West, notably in those beds which we have come to call the White River formation of Nebraska, because in those days Nebraska occupied all that northwest territory, including the present Dakotas and Kansas. It was a general term for that uninhabited region.

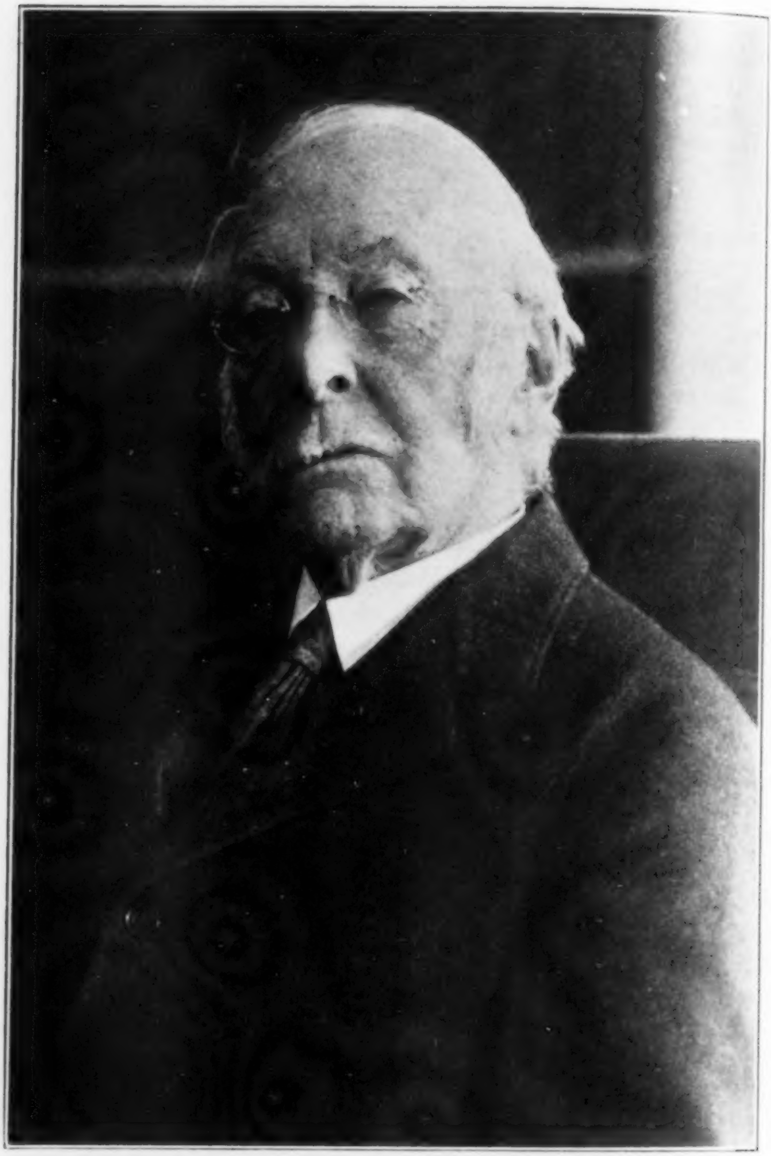
In the year 1849, I think it was, Dr. John Evans found some White River specimens and sent them to Dr. Leidy, and in the following year, an undergraduate of Princeton, Thaddeus Culbertson, who was afflicted with tuberculosis, had been ordered to go west by his physician, and he was advised by Professor Baird, who was then assistant secretary of the Smithsonian Institute, to this effect: "Go to the White River country and collect fossils and send them to Leidy, because Leidy is the only person in the country capable of dealing with them." Leidy published a number of papers on these fossils and then, in the year 1853, he gathered them together in one report, illustrated with the most beautiful lithographic plates I think that have ever been issued in any publication in America. They were made by a Swiss artist, Sonrel, and they are unrivaled in their beauty of execution. Still more comprehensive was the great work published by the academy here in the year 1869, and those works form the starting point, really, of our knowledge of the White River fauna, and we have been collecting there ever since. Expeditions go into the White River country every season, I think, but we have done very little except to fill in the sketch which Leidy outlined..

He was the first man to show, for instance, not only that there were native horses in America, and, as we were told this morning, the first American rhinoceroses. He found also the first of American camels, and it was shown that the line of the camels began here in America.

My time is running out and I want to say just one or two more words. Next followed his work on the Bridger formation, the older beds of southwest Wyoming, and in the year 1877 he made his first visit to those Bad Lands and wrote a most vivid description of the Bridger Basin. It shows what a geologist was lost in him, if he had had the opportunity to turn to that kind of work. He then published the first of the works on Upper Cretaceous reptiles in Montana and the Upper Cretaceous fauna of the Atlantic Coast. There is mounted in the museum of this academy the first dinosaur skeleton that was ever put together in America. There is a lot of conjectural restoration and the head—in fact, as put up by the late Waterhouse Hawkins, an English scientific man, not so very scientific, but known more for being artistic—the head is entirely grotesque, but it is the first time an attempt to put a dinosaur together was ever made and it is Leidy's dinosaur, described and named by him, from the Cretaceous of New Jersey. So, you see, he laid broad and deep the foundation upon which the great structure of American vertebrate paleontology has been erected. Why did he get out of it? There were two different reasons. They were both true, and one of them he didn't care to speak about much in public. Those of us whose hairs are gray or whose heads are devoid of hair will remember the bitter quarrel that existed—a feud—between Professor Marsh and Professor Cope. They succeeded Leidy to a certain extent and they hated one another with the most deadly hatred. Both were rich men, and so they diverted the stream of fossils from Leidy. As Leidy told Geike, he said, "I have got to get out because when anybody used to find a fossil they used to send it to me and I got it for nothing. Now to-day Cope and Marsh pay money for such things and I can't compete with their long purses and so I have got to get out." That is true enough. That is one reason. There was another reason which I don't think he ever expressed in public, but he did express it to me, and I have no doubt to many other people. He said, "I can't stand this fighting. It disgusts me and I am going to drop Paleontology and have nothing more to do with it because of the way Marsh and Cope are in each other's wool all the time." And yet he couldn't stop. And some of the last work of his life was done in describing not only the fossils from the phosphate beds of South Carolina, but later still the work

which he did in Florida. And it is a very interesting coincidence that one of the first things he described from the White River bed of Nebraska was the saber-toothed cat, tiger as we call him, which was about two feet high, and one of the last things he did before his death was to describe the termination of that tiger in the great beast it became in the Pliocene.

Now this is a most inadequate sketch of a vast subject, but you will easily see that you can not put a quart in a pint pot, and you can't describe a great character in fifteen minutes, but I hope I have left with you the impression that Leidy's work is the foundation upon which all subsequent American vertebrate paleontology was built.



—*Wide World Photos*

DR. CHARLES W. ELIOT

President emeritus of Harvard University, whose ninetieth birthday was celebrated on March 20 under the auspices of the Harvard Alumni Association, the Associated Harvard Clubs and an honorary committee of citizens, of which President Coolidge is chairman.

# THE PROGRESS OF SCIENCE

By Dr. EDWIN E. SLOSSON
SCIENCE SERVICE, WASHINGTON

## CHEMICAL MESSENGERS

What system of government prevails in this body of ours? Is it an autocracy, the one-man rule, such as prevailed in the primitive state and still survives in the army? Or is it a democracy, the equal power of all in politics, regardless of their qualifications, such as is now regarded as the ideal? Or is it an oligarchy where the superior cells and organs manage the inferior?

Strange to say, no system of human government has yet been devised that approaches the organization of the animal organism in character—or in success. The millions of cells, the hundreds of muscles, the dozens of organs, with their infinitely varied powers and functions, are kept in harmonious activity for the good of the whole by some secret system of mutual cooperation which man has not yet learned how to apply to his artificial organism, the state.

The conscious ego can not claim to be the dictator of the physiological realm which he calls his body. He is not even a premier, but merely a foreign minister. He has a certain control over imports and exports, but the department of the interior is mostly beyond his jurisdiction. It is his business to keep the body out of fights with others that might result in a stab in the heart or a punch in the stomach, but he is not entrusted with such essential functions as keeping the heart pumping and the stomach digesting. For, important as the mind may think itself, it sleeps at its post for a third of the twenty-four hours and is liable to occasional fits of forgetfulness at any time. It is not the brain that mobilizes the white blood corpuscles whenever an army of microbes invades the body through a breach in the outer wall. Sight is not sharp enough to see a microbe, and even if the brain suspected an invasion it would not know how to conscript the corpuscles and dispatch them to the front.

All these millions of living cells in brain or brawn or bone have to be kept supplied with food, water and air, in amount depending on how they are working and how fast they are growing. The temperature of every part of the body has to be kept constant no matter whether the weather is cold or warm, and the ashes must not be allowed to accumulate in any cell.

Now one would think that such a marvelously complicated coordination of interdependent activities would require a strict system of bureaucratic centralized government. But, on the contrary, the central government, if there is such, has little or nothing to say about most of the physiological processes. The orders to an organ come from below rather than above. For instance, if an over-worked muscle needs more oxygen, it does not petition headquarters, but sends orders direct to the heart and lungs to speed up the pumping. If a gang of structural bone workers want more lime or phosphate they do not bother the boss about it, but dispatch a message straight to the supply department to import some.

How these multifarious messages could be carried was long a mystery, but is now being solved. There are two ways of intercommunication in the body just as there are in the outside world, telegraph and mail. In a

telegraphed message nothing travels except the electrical impulse, but in the postal service a material message, the letter, is transmitted. Inside the body signals may be sent by the nerves, which play the part of telegraph wires, but it has recently been discovered that there is another and more general system of intercommunication by means of chemical substances sent around through the blood, like letters. Professor E. H. Starling, of London, pointed out the importance of these eighteen years ago and named them "hormones," which is Greek for "messengers," and since then many of them have been discovered and some of them manufactured.

The two systems of transmitting orders supplement each other like telegraph and mail. For instance, a man sits down at a dinner table. The eye signals by way of the nerves, "I see food," and a minute later comes confirmation from the nose, "I smell it." At once the saliva begins to pour into the mouth and the gastric juice into the stomach to prepare for the first stages of digestion.

Sometime later when the stomach has finished its work, three other digestive fluids have to be in readiness. These are secreted by three separate organs, the pancreas, the liver and the intestinal glands, and all these have to be notified to get busy as soon as the first food passes out of the stomach.

In this case the message is conveyed by a hormone called "secretin" which within two minutes after it has been sent into the blood stream sets the three organs to preparing their particular digestive juices.

If we get angry or scared, the body has to be put into a state of preparedness for fight or flight, whichever the high authority decides upon. But either will require an extra supply of energy, so the suprarenal glands, without waiting for special orders from headquarters, send a chemical messenger to the heart to pump harder and to the liver to release more sugar into the blood so that no muscle shall be short of fuel in this emergency.

How the sugar is handled depends on another hormone known as "insulin" which has lately been prepared in a form that may be used by diabetics whose pancreas does not work well.

Still more recently comes the announcement of the extraction of a pure and extremely powerful form of "pituitrin," the secrẹtion of the insignificant pituitary body, that controls the kidneys and capillaries.

The chemist is now able to make "thyroxin," which is secreted by the thyroid glands, and a minute daily dose of this may, as Dr. Starling says, effect "the conversion of a stunted, pot-bellied, slavering cretin into a pretty, attractive child."

It is these chemical messengers which in infinitesimal amounts determine whether we shall be tall or short, dark or fair, handsome or ugly, active or sluggish, alert or stupid, cheerful or melancholy, and it is the aim of the chemist to learn how to make them, or perhaps similar substances of even greater potency so that he can acquire absolute control over the workings of the human body.

## THE USE AND MISUSE OF EDUCATION

Learning is a tool. Its value depends on what is done with it. Give a jack-knife to a boy and he may whittle wood or cut his fingers with it. The knife is neutral. Much of elementary education must be merely formal, the giving of tools to children. The three R's are nothing in themselves. They are merely the keys to the knowledge of good and evil. Whether they prove beneficial

or injurious to the student depends on what use he makes of them. Reading the wrong books may make a man worse than an ignoramus. Learning writing may qualify him for forgery and learning arithmetic for swindling. The value of a ship's load can not be calculated by the inspection of the Plimsoll mark. The value of an education depends more on the character of the cargo than on the capacity of the cranium that carries it. Neither an information test nor an intelligence test can determine what the man's mind will be worth to the world.

In repeating these hackneyed observations, I am not presenting an argument against the alphabet, but I am pleading for its proper employment. Illiteracy is always a bad thing, but literacy may be an evil thing.

Opportunity does not insure progress. Christian missionaries like Livingstone rejoiced over the opening up of Africa by commerce and communications because they naturally and naïvely assumed that it meant the spread of Christianity. On the contrary, it led to an unprecedented spread of Mohammedanism, their most formidable foe.

If science teachers merely teach their students to use the appliances of science and fail to train them in the scientific way of thinking they may find the intellectual aims of science defeated by the machinery of science. The printing press contributes to the spread of superstition and obscurantism as well as to the spread of science. The newspapers publish a lesson in astrology more often than a lesson in astronomy. In our books and magazines fiction vastly outweighs fact. By means of the radio Voliva's argument for a flat earth is broadcasted from Zion City all round the world.

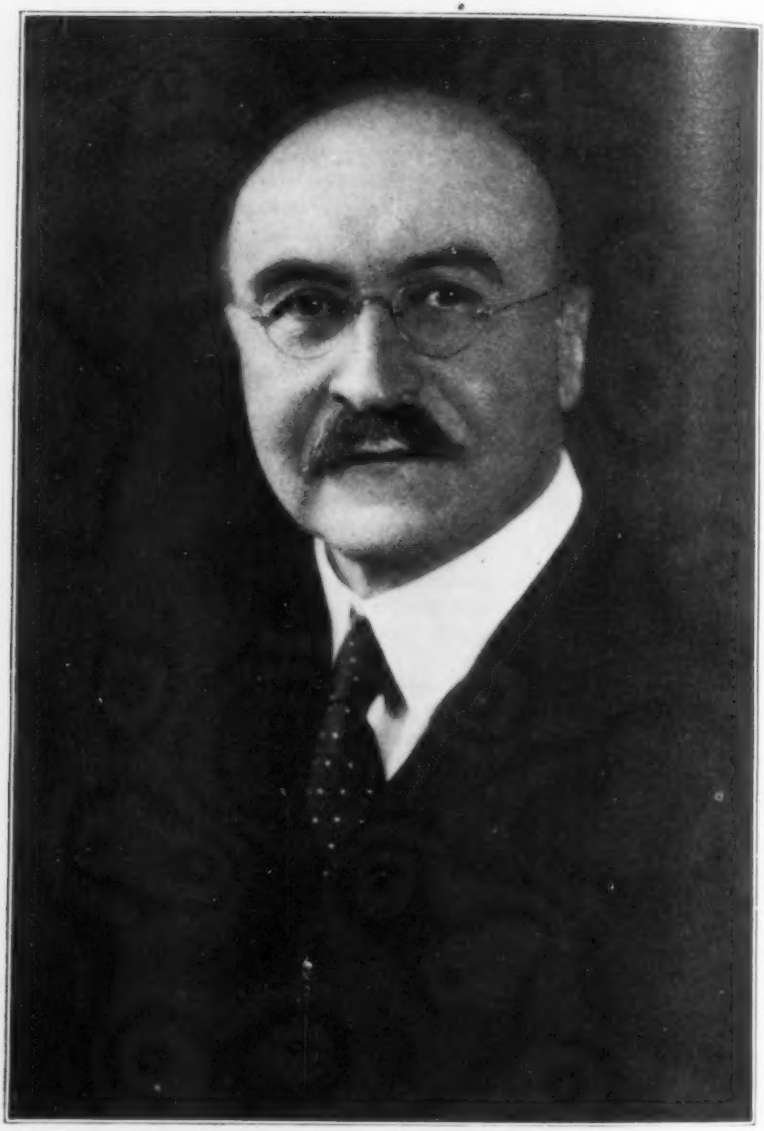
The main object of education in a democracy is not to teach the students how to vote right, but to train them how to think right. Under any form of government, in an autocracy no less than in a democracy, the real power lies in the people, and it is their individual every-day conduct, guided by their personal beliefs, that determines whether the nation shall advance, stagnate or retrograde.

Politics is not yet a science and there are many ways of reaching the same result. In science there is only one truth but an infinitude of falsehoods. A problem has a single solution. An unwise popular vote on a political question may bring a temporary calamity upon a nation, but an unsound popular opinion on a scientific question may bring permanent ruin to a race. It would not have mattered much if the legislature of Indiana had passed the bill fixing a fictitious value of Pi, but it would have made lots of trouble if the engineers and mathematicians of the world had adopted the wrong figure. The fate of the nation depends less on how the people cast their ballots than on how they combine their chromosomes.

## THE FOURTH STATE OF MATTER

Now that the kids on roller skates are talking familiarly about vacuum tubes and electron streams, and not merely talking about them but playing with them, it is interesting to turn back the pages of history to the time when these things were new, and nobody in the world perceived their significance but one man and he but dimly.

We do not have to turn back very far, only 45 years, when William Crookes exhibited the vacuum tubes that were afterwards known by his name. He found that when he exhausted the air as completely as possible from a glass tube and then passed an electric current into it by



DR. L. H. BAEKELAND

Elected president of the American Chemical Society. Dr. Baekeland, who is honorary professor of chemical engineering in Columbia University, is known for his discoveries of velox paper and bakelite and for his other important work in industrial chemistry.

platinum poles stuck through the glass that there proceeded from the negative pole or cathode a curious kind of a ray. Where the ray started from the cathode disk it was for a space dark and invisible, further on it became a beam of bluish light and where this struck the opposite side of the tube it made a greenish glowing spot on the glass. That this ray was not ordinary light he proved by holding a magnet up to the tube, for the cathode ray was curved out of its course by the magnetic force and could be turned in any direction, instead of going obstinately straight ahead as a common light ray does in a vacuum.

Such experiments with the "Crookes' tubes" amused the public and amazed the scientists. Everybody admired Crookes' skill as a glass blower and wondered how he got a little windmill inside a sealed tube, even as the King of England wondered how the apple got into the dumpling. But when Crookes claimed that he had in his tubes "a fourth state of matter" and a new kind of radiation and a connecting link between matter and energy his scientific colleagues were skeptical. They felt that he had gone too far, had become a monomaniac on the subject, had, in short, got vacuum on the brain. There were only three states of matter, as everybody knew, solid, liquid and gaseous. To have a fourth state the atom must be split and the very name of "atom" meant something that could not be split. This man Crookes never had a university education anyhow, and he was the son of a tailor, and he said he had seen spirits in the séance room, and altogether it was a bit cheeky of him to bring forward such upsetting ideas on such empty evidence as a vacuum tube.

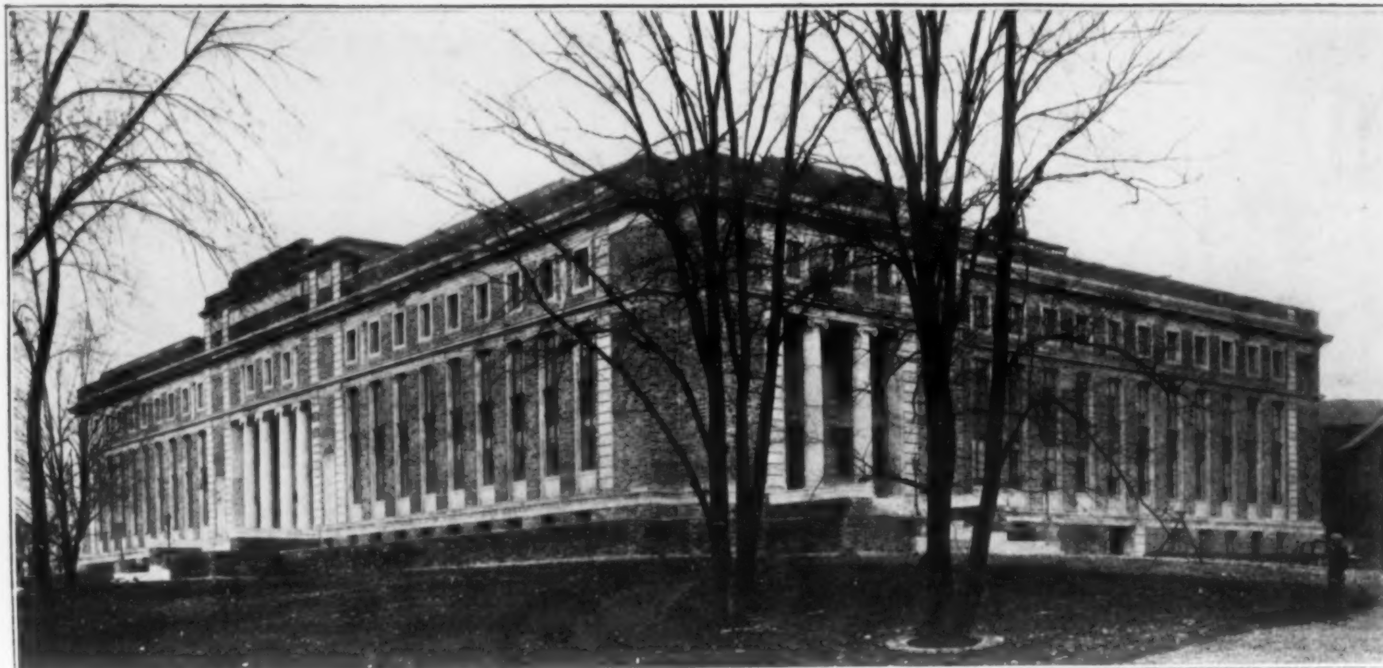
But Crookes always had the courage of his convictions, and in this case proved himself a true prophet. Two passages quoted from his 1879 addresses in the "Life of Sir William Crookes" by Fournier d'Albe, just published, will show how astonishingly he anticipated the views of the twentieth century:

"The phenomena in those exhausted tubes reveal to physical science a new world—a world where matter exists in a fourth state, where the corpuscular theory of light holds good, and where light does not always move in a straight line; but where we can never enter, and in which we must be content to observe and experiment from the outside.

"In studying this fourth state of matter we seem at length to have within our grasp and obedient to our control the little indivisible particles which with good warrant are supposed to constitute the physical basis of the universe. We have seen that in some of its properties radiant matter is as material as this table, whilst in other properties it almost assumes the character of radiant energy. We have actually touched the border land where matter and force seem to merge into one another, the shadowy realm between known and unknown, which for me has always had peculiar temptations. I venture to think that the greatest scientific problems of the future will find their solution in this border land, and even beyond; here, it seems to me, lie ultimate realities, subtle, far-reaching, wonderful."

Yet all these were, when no Man did them know,
Yet have from wisest Ages hidden beene;
And later Times things more unknown shall show.
Why then should witlesse Man so much misweene,
That nothing is, but that which he hath seene.

We now know that the cathode ray of Crookes is, as he said, corpuscular and not vibratory, for it consists of a stream of electrons, which are "the



Baker Laboratory of Chemistry, Cornell University

Erected by the gift of Mr. George F. Baker, at a cost of $1,500,000, and recently dedicated.

ENTRANCE HALL, BAKER LABORATORY OF CHEMISTRY

little indivisible particles" that "constitute the physical basis of the universe," and they do indeed belong to the borderland of matter and energy. They are atoms of electricity and sub-atoms of matter. They change their mass when they change their motion, and where free-flying electrons strike solid matter they start a stream of energy in the form of waves, what we call the "X-rays." If Crookes had only happened to lay a photographic plate holder opposite the green spot where his cathode ray struck the glass, he would have anticipated Roentgen in the discovery of the X-rays by some seventeen years.

But it was glory enough for one man to have revealed the cathode rays inside the sealed tube even though he failed to follow their course outside. Thanks to Sir William Crookes, Londoners can now listen in on Pittsburgh concerts and he foretold the means and method of wireless telegraphy as early as 1892, five years before Marconi sent his first message by radio.

### THE FERTILITY VITAMIN

The two chief characteristics of life are growth and reproduction, the magnification and the multiplication of living beings. Essential for both is food. But what kinds of food? This is the question that is being gradually solved by feeding experiments on man and lower animals carried out under chemical control by hundreds of investigators during the last quarter century.

The first thing found out was that there were four main essentials in food: (1) proteins, such as the casein of milk, (2) carbohydrates, such as sugar or starch, (3) fats and (4) certain mineral salts, such as calcium phosphate.

But when these four food factors were prepared chemically pure and mixed together they were found not to form a complete and satisfactory

diet. The animals fed on it failed to grow, or showed certain symptoms of disease. Evidently there was a lack of something or some things. This was a puzzle, for whatever they might be they were too delicate in structure for the chemist to extract, and too minute in amount for him to weigh. They have been named the "vitamins" and distinguished provisionally by the letters of the alphabet. Although nobody has yet seen a vitamin, we know pretty well which foods contain them and what happens if they are wanting. They are defined negatively, as salt was defined by the schoolboy: "Salt is what makes potatoes taste bad when you don't put any on."

If you don't have Vitamin A you are likely to get a certain sort of sore eyes. If you don't have Vitamin B you are likely to get beri-beri. If you don't have Vitamin C you are likely to get scurvy. If you don't have Vitamin D you are likely to get rickets.

With the four main food factors in purified form and the four vitamins pretty well identified, the investigators could now make up an artificial diet on which animals, white rats being usually used, would grow as big as those that were fed on natural food. They were as handsome and happy as any, and lived as long; but they failed to provide for the continuance of the ratty race. Their offspring were few and infrequent, or none and never, which is contrary to the custom in rat families. But the investigators, instead of accusing the rats of race suicide, surmised that this failing might be a deficiency disease, so they set themselves to find the missing vitamin. And they have found it, or at least they have found that there is one. Herbert M. Evans and Katharine Scott Bishop, of the University of California, and Barnett Sure, of the University of Arkansas, have carried on experiments leading to the same conclusion.

It is found, for instance, that on a diet composed of milk casein for protein, cornstarch for carbohydrate, lard for fat, and the proper mineral salts, with the addition of a little butter for Vitamin A, yeast for B, orange juice for C, and cod-liver oil for D, the rats grew normally and thrived, but they failed in fertility. Increasing the amount of the diet or of any of its constituents did not remove the deficiency, but the addition to the dietary of a little lettuce or rice, even the polished kind, enabled the rats to reproduce. Four successive generations have been raised on such a synthetic diet.

It is interesting to recall that rice, which had so marked an effect in these experiments, has a high reputation in the Orient for the promotion of fertility. This significance survives in our marriage customs to-day, and we often see on a depot platform a bridal party showering the young couple with rice in spite of their attempts to evade it. Other foods found to contain this anti-sterility factor are yellow corn, rolled oats, velvet bean-pod meal, dried alfalfa, field pea seedlings, egg yolk and cooked meat. It is missing from milk.

Evans and Bishop have found that the male as well as the female is affected by the lack of this substance, and they have been able to extract it from favorable foods by alcohol and ether. When the extract is added to the "pure food" diet on which the rats were sterile, they gain the power of reproduction. These investigators call the substance, as Roentgen called his rays, "Vitamin X," but Professor Sure proposes to promote it in the alphabet and class it as "Vitamin E."